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THE PERSONAL EQUATION.

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IN an attempt to explain clearly some of the phenomena which have led to the consideration of what astronomers call the "personal equation," it will, perhaps, be most advantageous to consider the subject somewhat in an historical manner. In this way we shall, it is true, lose something in directness, but it will assist in gaining a definite conception of the whole subject if we consider it in the order in which astronomers have been forced to do.

To make the meaning of the term plain, it will be necessary to premise a brief account of the methods of observation with astronomical instruments, and of some of the refinements which have gradually been found necessary in these methods.

Nearly every astronomical observation has for an object to fix the *relative* position of two bodies *at a given time.* If, then, a second observation of a similar kind is made, these two, taken together, will suffice to give some idea of the apparent relative motion of one body, referred to the other. If, for example, the design is to determine the orbit of a new comet, the mode of proceeding is, or might be, something as follows: Some star, whose place is known (or whose place is subsequently determined), is chosen in the vicinity of the comet, and the distance of the comet from this star is measured. This may be done in several ways, by a sextant, with which we can measure this distance directly, or, more usually, by one of the fixed instruments of an observatory, with which we can determine two things: 1. The distance of the comet east or west of the star; and, 2. Its distance north or south of it. The distance north or south is usually determined by a direct measure of the celestial arc included between the respective parallels on which the star and comet are at a given time; while the distance east or west is usually measured by the interval of time required for the earth's rotation to carry a body from the

meridian of the star to that of the comet. To make this measure, it is customary to fix in the focus of the telescope some uneven number of fine filaments of spider's-web at (say) equal distances apart, and to allow the telescope to remain fixed while the diurnal rotation of the earth carries the body first to be observed into the field of the telescope and slowly across this. As it crosses each of the threads, the time at which it is exactly on the thread is noted. Now, when the second body enters the field of the telescope (which is supposed to remain fixed in its former position) the times of *its* passage over the various threads are noted.

The mean of the times for the first body gives the time at which this body was on the middle thread (these being at equal intervals), while the mean of the times for the second body gives the corresponding time for the second body, and the difference of these two times gives evidently the distance which one of them is, east or west, of the other, expressed in *time*. This may be easily reduced to *degrees*, etc., by the rule that twenty-four hours is equal to 360 degrees.

If it were possible for an astronomer to note the *exact* instant of the *transit* of a star over a thread, it is plain that one thread would be sufficient; but, as all estimations of this time are, from the very nature of the case, but approximations, several threads are inserted in order that the accidental errors of estimations may be eliminated, as far as possible. The method of making these estimations will be better understood from the two following figures, 1 and 2. Fig. 1 represents

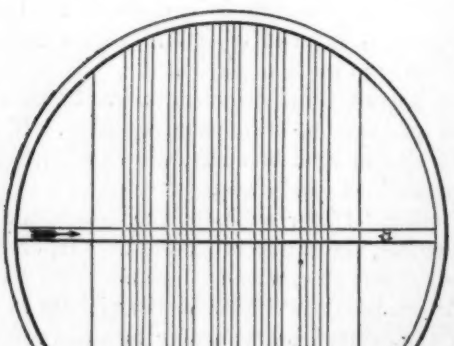


FIG. 1.—TRANSIT-THREADS IN TELESCOPE.

the *reticle* of a transit-instrument as it would be viewed by an observer, where twenty-five threads are placed arranged in groups or *tallies* of five. The star may enter on the left hand in the figure, and may be supposed to cross each of these wires, the time of its transit over each

of them, or over a sufficient number, being noted. The method of noting this time may be best understood by referring to Fig. 2.

Suppose that the line in the middle of the figure is *one* of the transit-threads, and that the star is passing from the right hand of the figure toward the left: if it is on this wire at an exact second by the clock (which is always near the observer, beating seconds audibly), this second must be written down as the time of the transit over this thread. As a rule, however, the transit cannot occur on the exact beat of the clock, but at the seventeenth second (for example) the star will be on the right of the wire, say at *a;* while, at the eighteenth second, it will have passed this wire and may be at *b*. If the distance of *a* from the wire is six-tenths of the distance *a b*, then the time of transit is to be recorded as — *hours* — *minutes* (to be taken from the clock-face), and seventeen and six-tenths *seconds;* and in this way the transit over each wire is observed. This is the method of "eye-and-ear" observation, the basis of such work as we have described, and it is so called from the part which both the eye and the ear play in the appreciation of intervals of time. The ear catches the beat of the clock, the eye fixes the place of the star at *a;* at the next beat of the clock the eye fixes the star at *b*, and subdivides the space *a b* into tenths, at the same time appreciating the ratio which the distance from the

FIG. 2.—PASSAGE OF STAR ACROSS THE THREAD.

thread to *a* bears to the distance *a b*. This is recorded as above. Now, if the action of the eye and the ear and the coördinating action of the brain (which must associate some spot in the field of view with some second) were all instantaneous in their action, the phenomenon of personal equation would not exist. As a matter of fact, when the clock beats and the star is really at *a*, the mind refers it to some point farther on in the field as *a'*; and when the clock again beats, the star, which truly is at *b*, is by the mind referred to a point *b'*. The distance *a b* is the same as *a' b'*; but the distance from the thread to *a* is greater than the distance from the thread to *a'*. Hence, instead of recording the time of transit as 17ˢ.6, an observer, whose habit is correctly represented by the figure, might record this time as 17ˢ.4, and the correction + 0ˢ.2 would be required to be applied to his times of transit to reduce them to the exact truth: + 0ˢ.2 is then his *absolute* personal correction. But, in general, we have no means of determining where *a* and *b*, in our field of view, are, and hence the knowledge of the *absolute* personal equation has to be gained by some special de-

vices, to be hereafter spoken of. A little consideration will show, however, that, although every transit observed by our astronomer is too early by 0ˢ.2, yet, in ordinary cases, this correction is of no account, provided only that it is constant. If he observes the star too early by 0ˢ.2, and the comet also too early by that amount, the *difference* in the times will be *absolutely* correct. But suppose one observer to note the transit of the star, and another that of the comet: each may have a peculiar habit, so that where one would note 0ˢ.2 too early, another might note 0ˢ.3 too early, and the difference of their *absolute* personal equations, 0ˢ.1, it would be necessary to apply to the observations of A to reduce them to homogeneousness with those of B. This difference of *absolute* personal equations is *relative* personal equation, which, when once truly known, enables us to reduce the observations of one skillful astronomer to what they would have been had another made them.

We say "skillful," because it is only among skillful observers that the phenomenon in question is truly found. In astronomical observations the senses are trained to a fine delicacy, and old observers acquire a constancy of habit which gives to their work a homogeneousness that is wanting in that of younger men.

We have given a brief account of the early method of estimating the time of a star's *transit* across a spider-line in the field of the telescope by the method of *eye and ear;* there is yet another method now in common use, which it is necessary to understand before we pass to the consideration of the means of determining personal equation.

This second method is the American or chronographic method; this consists, in the present practice, in the use of a sheet of paper wound about and fastened to an horizontal cylindrical barrel, which is caused to revolve by machinery once in one minute of time. A pen of glass which will make a continuous line is allowed to rest on the paper, and to this pen a continuous motion of translation in the direction of the length of the cylinder is given. Now, if the pen is allowed to mark, it is evident that it will trace on the paper an endless spiral line. An electric current is caused to run through the observing clock, through the pen, and through a key which is held in the observer's hand.

A simple device enables the clock every second to give a slight lateral motion to the pen, which lasts about a thirtieth of a second. Thus every second is automatically marked by the clock on the chronograph-paper. The observer also has the power to make a signal (easily distinguished from the clock-signal by its different length), which is likewise permanently registered on the sheet. In this way, after the chronograph is in motion, the observer has merely to notice the instant at which the star is *on* the thread, and to press the key at that moment. At any subsequent time he must mark some hour,

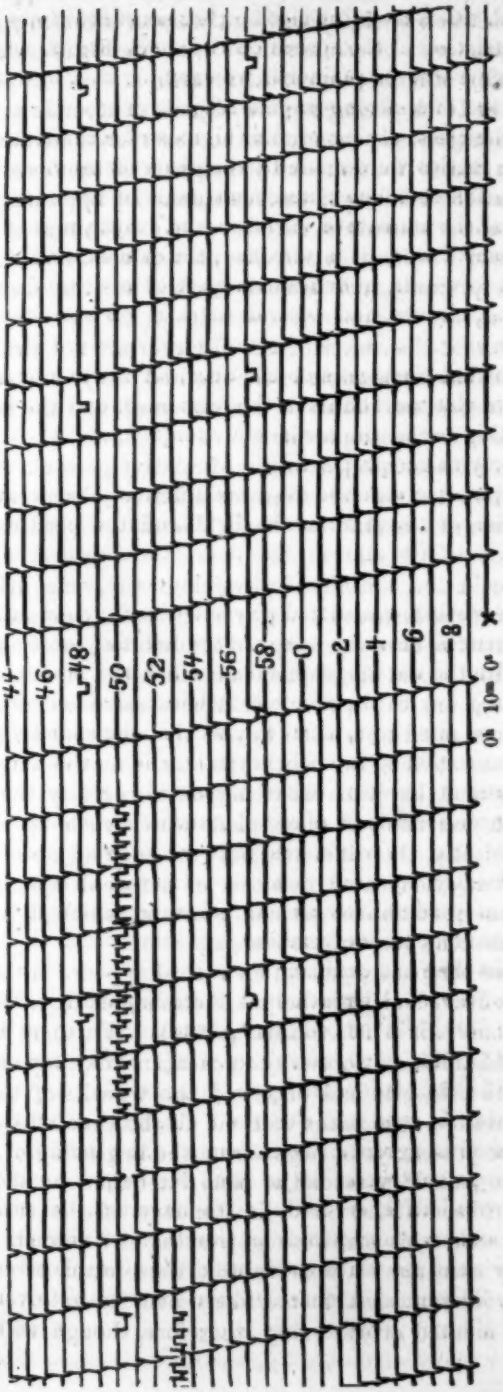


FIG. 8.—PORTION OF A CHRONOGRAPHIC RECORD.

The upper line of figures numbers the seconds; and as twenty of them are shown, and the cylinder revolves once in sixty seconds, about one-third of the length of the strip of paper is here represented. The middle column of figures numbers the lines, and, therefore, the minutes. The jogs in the lines are repeated so exactly at intervals of one second as to give the effect of columns across the paper. As explained in the text, it is all done by one pen, which makes one continuous line round and round, the machine making the regular jogs as a time-scale, and the observer making the irregular jogs at the instant the star reaches the thread; its time being thus exactly recorded.—ED.

minute, and second, taken from the clock on the sheet at its appropriate place, and the translation of the spaces on the sheet into times may be done at leisure. This will be plainer if we examine Fig. 3, which is a fac-simile of a portion of a chronographic record.

The marks of the clock giving regular signals of seconds are easily distinguished; the rattles were made by the observer to attract attention to an observation to follow: and the signals of the observer are seen at the times of the transits of various stars. By applying a graduated ruler to the sheet when it is unrolled, the exact times of transit can be determined to within a hundredth part of a second, provided we have some hour, minute, and second marked on the sheet as an origin of time.

It is quite plain that the senses of the observer are not strained to so great a degree in this method as in the method of eye and ear; the eye has but one thing to do, the ear is not occupied, and the hand has only to press the key at the proper time.

In this method, we see that the origin of relative personal equation is again in the different times required for different observers to coordinate the position of the star in the field and the position of the wire.

True personal equation, considered physiologically, must arise from the personal differences between observers when they note the *same* phenomenon. With the chronograph it is the habit of most observers to tap the observing key at the moment at which the star is actually *on* the wire. There are cases, however, where astronomers of some experience are accustomed to tap the key so that the sound of the tap shall come to the ear at the time when the star is on the wire. This seems an utterly wrong habit of observing, as it is really the record of an event which has not yet taken place which such an observer makes. Astronomically, the difference between such an observer and another observer may be treated as a case of personal equation, provided the habit described above remains constant, which it is probably less likely to do than the ordinary one.

The first case of personal equation on record appears in the "Observations" of the Rev. Nevil Maskelyne, Astronomer Royal for England ("Observations" for 1796, vol. iii., p. 339). We there find the following note: "I think it necessary to mention that my assistant, Mr. David Kinnebrook, who had observed the transits of the stars and planets very well in agreement with me all the year 1794, and for a great part of the present year, began from the beginning of August last to set them down half a second of time later than he should do according to my observations; and, in January of the succeeding year, 1796, he increased his error to eight-tenths of a second. As he had unfortunately continued a considerable time in this error before I noticed it, and did not seem to me likely ever to get over it and return to a right method of observing, therefore, though with reluc-

tance, as he was a diligent and useful assistant to me in other respects, I parted with him."

But time has its revenges, and Kinnebrook's observations are now used as well as Maskelyne's (*see* "Annales de l'Observatoire de Paris; Memoires," iii., p. 307), and they are probably about as free from accidental errors as his.

In 1822 Bessel examined this subject, and we find in the Königsberg observations of that year an account of quite extended experiments on personal equation.

Bessel, after quoting from Maskelyne's own report (see extract above), considers the subject at some length. He calls attention to the fact that the accidental errors in an eye-and-ear observation certainly do not exceed two-tenths of a second, and that a careful consideration of the observations of Maskelyne and his assistant shows that there may be an "involuntary constant difference" between the estimations of various observers which far surpasses the limits of possible accidental error.

In 1819 Bessel made a visit to the Seeberg Observatory, where he observed, on two nights, transits with Von Lindenau and Encke. These observations showed no personal equation between these three celebrated astronomers. In 1820 Dr. Walbeck and Bessel made several sets of observations at Königsberg, for the purpose of determining their relative personal equation, and the results of their work are given below:

						s.
1820, December 16th and 17th,	Walbeck later than Bessel					1.045
" 17th and 19th,	"	"	"	"		0.985
" 19th and 20th,	"	"	"	"		1.010
" 20th and 22d,	"	"	"	"		1.025
	Mean					1.041

Bessel says that this great difference was evident from the second day, and that no pains was spared by either of them to observe carefully; and that at the end of the series each was confident that it would have been impossible for him to observe differently, by so much even as a tenth of a second. Here, then, was an enormous difference—one almost incredible. To test the reality of the phenomenon, Bessel compared with Argelander, and found that Argelander was later than he by 1ˢ.223.

Bessel remarks that neither Walbeck nor Argelander had observed as much as he had with the transit-instrument, and he therefore used all opportunities for comparing his work with that of Struve, of Dorpat. He found that in 1814 Struve was later than himself by 0ˢ.044; in 1821, by 0ˢ.799; in 1823, by 1ˢ.021. Bessel now determined to arrive at some conclusion by studying this phenomenon under different aspects.

To this end Argelander and himself noted the times of 78 disap-

pearances or reappearances of a material object, and he found that Argelander was later than himself by 0ˢ.222. Again, in the observation of the occultations of stars (an instantaneous phenomenon), Argelander was slower than Bessel by 0ˢ.281. Here was some light: for it was now evident that not only had each astronomer a different habit of estimating time, but that this habit was only constant so long as the same phenomenon was observed; that a personal equation for transit observations would not serve for observations of occultations.

Bessel next investigated the question whether there was any difference in his own absolute personal equation in observations with a clock beating whole seconds, or with a chronometer beating half-seconds; he found that he observed 0ˢ.494 *later* when the clock beat half-seconds than when it beat whole seconds, while Argelander and Struve did not change their habits in this regard.

Bessel's whole investigation is very complete, especially when we consider that it was the first published research on a subject which had escaped attention until his time. The principal points established were:

1. A personal equation subsists in general between two observers.

2. For limited periods of time this equation is probably constant between two observers for the same class of work.

3. The absolute personal equation of any one observer varies with the class of observation; i. e., from transit observations to sudden phenomena like occultations.

4. The rapidity with which the star (in transit observations) traversed the field of the telescope had no influence on Bessel's personal equation.

Bessel does not seem to have supposed that there would be any different personal equation for stars and for the moon. This we now know to have been erroneous, and we shall see that the apparent velocity with which a star moves through the field of the telescope is also held by some observers to have an influence on the magnitude of their personal equation.

All of the preceding results referred simply to the personal equation between observers who were using the eye-and-ear method. As soon as the chronographic method of registering transits was introduced, it was seen that the personal equation became smaller. This is undoubtedly due to the smaller amount of work which the brain has to perform; the phenomena to be appreciated are, in this latter case, far more simple than in the former, and the effect of this is shown in the amount of personal difference.

We must now give a brief account of the ordinary methods for determining the amount of the relative personal equations of various observers, in order that we may proceed to the determination of the absolute equation, which is of great interest physiologically and psychologically, although not of capital importance to astronomy. As

we have seen, to reduce the observations of A to what they would have been if B had made them, it is simply necessary to know how much later B is in the habit of observing than A, and to apply this as a constant correction to A's work.

This may be done in practice by A and B observing the same star in the same telescope; A over the first ten wires (*see* Fig. 1), and B over the second ten.

A knowledge of the distances of the various wires from the middle wire enables us to compare A's work with B's, and A—B is the relative personal equation.

There is, however, a strong objection to this process: if personal equation is any thing, it is the difference between established *habits;* and, if A observes over ten wires, and then hastily rises to allow B to take his place at the instrument, both A's habits and B's are broken in upon, and the resulting personal equation is likely to be affected by this fact. In general, the way adopted is to allow A to observe several stars leisurely, and from them to determine the error of the clock; B does the same, and from his observations also a clock-error is found; the difference of these clock-errors, reduced to the same epoch, gives the relative equation of A and B.

Now if, instead of A registering his own observations on the chronograph (for example), we could have the star register its own transit, then B's observation, compared with this, would give at once an absolute equation. We cannot use the real star for this purpose; but several attempts have been made to construct an apparatus which should register the transit of an artificial star, which star could, at the same time, be observed. The principle of all of these machines is, in general, the same, and we will merely give a brief account of one which is now under trial by the Coast Survey.

The artificial star is produced by lamp-light falling upon a small hole in a blackened plate; this plate is given a motion laterally, and the small point of light passes from one side to the other of a plate of ground glass, upon which lines are ruled to represent the spider-lines of the *reticle.* As the artificial star passes each wire, an electric signal is recorded on the chronograph, and the observer can also record his signal; and thus on the same chronograph-sheet many observations of absolute personal equation can be permanently recorded. Any velocity can be given to the star, so that it may pass through the field of view as slowly as the pole-star, or as rapidly as a star at the equator.[1]

[1] The chief objection to this apparatus is, that there is a constant error in its indications; i. e., it can never be adjusted so as to give its signal at the exact moment of transit, but it is always too soon or too late. This is sought to be eliminated by allowing the artificial star to travel first from right to left, and then from left to right, and using the *mean* of the two determinations. It is still a question whether the *observer's* habit is the same no matter which way the star is moving.

An apparatus similar to this was invented and used by Wolf, of the Paris Observatory, and we owe to him much the fullest account of personal equation which we have. We cannot do better than to give a brief abstract of his memoir ("Mémoires de l'Observatoir e de Paris," tome viii., p. 153), as the results obtained by the American device have not been made public.

His first experiences showed him that his absolute personal equation, when he used the chronographic method of recording, was extremely small (from *three* to *four* hundredths of one second); and, although this was an interesting fact, yet the very smallness of this equation showed that it was hopeless to attempt to discover the *laws of variation* of so minute a quantity.

These laws would be masked by the accidental errors: so that all the observations of M. Wolf have been by eye and ear. It should be stated that M. Wolf is an observer of experience. In his own experiments he proposed to himself to determine the effect on his equation—

(*a.*) Of the position of the observer (sitting or standing, etc.).

(*b.*) Of the magnifying power of the telescope.

(*c.*) Of the direction of motion of the star (i. e., whether from right to left, or the reverse).

(*d.*) Of the brightness of the star.

His personal equation he found was, at first, about $+0^s.3$; and in a short time this fell to $+0^s.1$; this was undoubtedly due to the fact that the observer felt in what direction his observations had to be modified, in order to bring them nearer to the truth, and that he unconsciously so modified them. This, however, did not continue without limit; his personal equation remained, for all the time he observed, at this lower limit, and this fact gave him the first clew to the physiological explanation of the phenomenon.

M. Wolf finds that the brilliancy of the star has no sensible effect on personal equation, a conclusion identical with that derived by Mr. Dunkin, of the Royal Observatory at Greenwich ("Monthly Notices, Royal Astronomical Society," vol. xxiv., p. 158).

With regard to the influence of the direction of motion of the artificial star, M. Wolf finds in his own case a mean constant difference of $0^s.04$ obtained from over 400 transits: this he subsequently explains by the fact that, if his right eye be fixed on two dots equidistant from a line drawn on a sheet of paper, one of these dots always appears nearer to the line than the other by a small quantity. This, of course, is a defect in the symmetry of the eye, and it is quite a common defect, which probably many of the readers of THE POPULAR SCIENCE MONTHLY have, perhaps without knowing it.

The influence of the apparent velocity of the star Bessel states to have been nothing in his own case, provided the star was situated more than 20° from the pole. Wolf's experiments do not agree with this, and he confirms the researches of Dr. Pape and of Dunkin.

Pape finds (*Astronomische Nachrichten*, vol. xliv., p. 179) that the error of a transit observation is composed of two parts: one is constant, and the other depends on the polar distance of the star. Dunkin likewise considers the probable error of a transit observation as depending upon the polar distance of the star, and Wolf's experiments corroborate these results, and show that his own personal equation became larger as the velocity of the star increased. It is evident that this rule must be held true only within limits, and probably these limits are not very far apart. Wolf further made experiments to determine whether the position of the observer affected his personal equation, and he concluded that, for his own case, there was no effect due to this cause. It is probable that most astronomers would differ with Wolf in this respect: observers of double stars, especially, have noticed a constant influence in their measures due to the position of the head.

After having recited the results of his experiments, M. Wolf comes to the consideration of the really important question, "What is the origin of the phenomenon known as personal equation?" Before he discusses this, he considers the remarkable personal differences between Bessel and other astronomers which we have noticed, showing that this is undoubtedly the largest personal equation on record, and expressing his opinion that it was really due to an erroneous counting of the whole seconds, and that the fractional part of his enormous personal equation with Argelander (1ˢ.223) was alone a case of true physiological personal difference. Let us recall the fact that Bessel and Argelander differed in observations of sudden phenomena only by 0ˢ.222, or 0ˢ.281; and again, that Bessel observed transits with a chronometer beating half-seconds so much as 0ˢ.494 (nearly a whole beat) later than with a clock beating seconds; and it seems impossible to avoid Wolf's conclusion that Bessel counted his seconds differently from other observers. The only thing which militates against this theory is, that Bessel must have examined this question of enumeration himself; and again, that, in two nights' observation with Von Lindenau and Encke, he found no signs of personal equation. Encke, however, in speaking of this large personal equation of Bessel's, says that there is no doubt that he had a different method of counting the strokes of the clock from other observers. M. Wolf, too, mentions the case of an assistant at the Paris Observatory, whose transit observations were earlier by one second than those observed by his fellow-assistants (Bessel's habit), but, in this case, a few experiments on artificial transits sufficed to show him that his habit was wrong, and led him to change it.

The opinion of most astronomers has been, that personal equation is not purely a physiological phenomenon, but likewise a psychological. The time required for the sound of the clock to reach the observer's brain, and the time required for the light to pass from the image of

the star, so as to excite the nerves of vision, are both very small: it is the coördinating power of the brain that works slowly—and absolute personal equation is largely the measure of the time required for the brain to superpose two different sensations, to coördinate impressions derived from different sets of nerves.

This view M. Wolf combats, and maintains, on the contrary, that the phenomenon in question is purely physiological, and arises from the duration of the luminous impression of the image of the star on the retina. To prove this, he has applied his apparatus to the observing of transits in which the seconds of the clock were not marked by audible beats, but by flashes of light appearing in the field of the telescope.

In this case, and also in the case where the seconds of the clock were not heard, but were marked by light taps on his hand, his equation remained almost constant (*see* table):

The second marked by sound,	$: \epsilon = +0^s.10$	80	observations.
" " " " sight,	$: \epsilon = +0.08$	80	"
The second marked by sound,	$: \epsilon = +0^s.11$	80	"
" " " " feeling,	$: \epsilon = +0.11$	80	"

This table seems to bear out M. Wolf's view; but, in this connection, it will be interesting to refer to a paper by Mr. T. C. Mendenhall, of Columbus, which appeared in the *American Journal of Science*, vol. ii., p. 157. This gentleman says: "An attempt was made to determine the relative rapidity with which responses are made to impressions made upon the different senses. . . . Time is measured on a register similar to the astronomical chronograph, in which I have been able to move a slip of paper with great regularity at the rate of about one and one-half inch per second, the seconds being registered upon the slip by a seconds pendulum according to the electric method. The person on whom the experiment is being made is seated at a table, having his hand on a key; by pressing this, the time of the action is registered on the paper. I made an apparatus, by means of which the circuit is completed for an instant the moment that there appears at a circular opening, about three-fourths of an inch in diameter, a card, red or white, as I choose, which completely fills the opening. The subject is instructed to watch this opening, and to press the key immediately on seeing the card. The actual appearance of the card and his closing the circuit in response are marked on the slip of paper by two dots about one-fifth of an inch apart (two-fifteenths of a second). This is a measure of the time occupied by the somewhat compler operation of his perceiving the object, and acting in response to that perception. I introduce the exercise of judgment by giving him two keys, one for each hand, and by instructing him that, when a white card appears, he is to close with his right hand, and when a red card appears, with his left. . . . According to the same general plan, I made trials concerning the sense of hearing. I arranged that, by

pressing upon the key, unseen by the subject, I could at the same time close the circuit and produce a clear and distinct sound, upon hearing which he made a response registered as before. I connected my apparatus with the key-board of a piano-forte in such a way that I was able to introduce an exercise of judgment in the comparison of two tones differing in pitch much or little, as I chose. . . . With different persons, as many as 2,000 individual trials have been made, and the errors of experiment eliminated as far as possible by averages. . . . As was anticipated, different individuals furnished, in some cases, strikingly different results, but, in general, they all followed the order given in the table:"

CASE OF A. G. F.	Time in seconds.
Response to appearance of a white card	0.292
" " " " an electric spark (in the dark)	.203
" " sound	.138
" " touch on the forehead	.107
" " " " " hand	.117
" when required to decide between white and red	.443
" " " " " " tones C and E	.335
" " " " " " C and C above (octave)	.428

One cannot but be struck with the additional time required when the phenomenon to be observed becomes even slightly more complex. This is evidently not entirely a physiological effect, but is truly psychological in part. Just what bearing this has on the question of the cause of personal equation it would be difficult to say: at the same time we must admit that the slightest additional exercise of judgment requires additional time. This is forcibly shown by the smallness of chronographic personal equation as compared to eye-and-ear-equation.

Let us now consider personal equation in things other than the estimation of time. We stated that the distance of one star, north or south of another, was usually measured directly; i. e., by graduated circles for large distances, and with micrometers for small ones. Prof. Coffin, now Superintendent of the American Ephemeris, has shown that in his own case, and in the case of two other observers, at the United States Naval Observatory of Washington, a marked personal difference appears in the observations of *a* Lyræ, and one or two other stars which pass near the zenith of Washington, *depending on the direction in which the observer faced*, whether north or south. It is plain that a star near the zenith may be observed as a south star or as a north star, and it appears that each position gives a different polar distance to the star: the difference of polar distance is small but constant.

In reading microscopes, and, in short, in performing any operation where the senses are strained to appreciate small differences of time, space, or position, and particularly where the judgment has to be exercised, personal differences are present. In general, these are constant with the same observer, and in astronomy they are usually eliminated in the determination of the zeros. For example, if an ob-

server reads the microscopes of a Transit Circle habitually too large, when he is determining the zenith-distance of a star, it is likewise his habit to read them too large when determining the position of the zenith-point from which zenith-distances are counted; and the resulting quantity is likely to be free from all but accidental errors.

Occasionally there arise cases where these differences (in the same observer) are not eliminated, but multiplied.

In the measurement of a base-line, for example, the various rods are brought into contact under a microscope: if an observer judges these rods to be in contact when they are not, it is evident that his error, originally small, will augment with the number of contacts, and it may become serious.

In the comparison of the national standards of length, undertaken by the English Ordnance Survey, an annoying case of personal difference was found.

These comparisons were made by bringing a movable cross of spider-lines to bisect one of the lines engraved on the various bars, and it was found that Captain Clarke, R. E., and Quartermaster Steel, R. E., who made the greater number of comparisons, differed in their estimation of a bisection by a constant amount which was annoyingly large: so that "the probable error of the final results is nearly double what might be expected from errors of observations only." This error cannot be eliminated, and it still remains in the published results.

We must constantly bear in mind that the quantities of which we have all along been speaking are extremely small, and that in fact they are masked by accidental errors for inexperienced observers in most cases. Still they exist, and they are among the most curious of phenomena: their careful study would well repay physiologists.

We can never be sure we have eliminated them so long as the human mind or body is a part of the machine by means of which we are comparing or registering events; and, just so long as mind or body is employed, we can be sure that personal differences will not only exist, but that they will vary from day to day. We must use for eliminating personality those values which are the best attainable, and assume these values to be constant over extended periods of time —weeks or months. In astronomy of precision, however, we have other errors to fear much more variable than personal equation, and it is to the elimination of these that attention should be directed. In other branches of research less exact in method, personality becomes of more importance, and an attentive consideration of its effects may be well worth while undertaking.[1]

[1] The writer has recently had occasion to examine drawings of the same nebula by different observers, with telescopes which are quite similar, and the enormous differences which exist in the representations show personal differences of the most marked kind, for nothing is more certain than that all the changes shown by the drawings have not taken place.

BACTERIA AND THEIR EFFECTS.

By L. A. STIMSON, A. M., M. D.

LIVING organisms, microscopical in size, of the simplest, most elementary nature, and moving freely in different liquids, have been known to observers for nearly two hundred years. Scientific classification and description were long impossible, on account of the meagre facilities furnished by the microscopes of the last century; but, during the last fifty years, the means of observation have been so much improved, and the number of observers has been so great, that the advance in our knowledge of microscopical bodies compares favorably with that in other branches of science. This advance has been greatly stimulated by a tendency to see in low vegetable or animal organisms the exciting cause not only of fermentation and decomposition, but also of many diseases. Pasteur's researches into the nature and causes of fermentation, the lectures and publications of Tyndall and Huxley, and the bitter discussions about spontaneous generation, have made us all familiar with the names *bacteria*, *vibriones*, and *micrococci*, or *microzymas*, with which we associate the idea of microscopi-

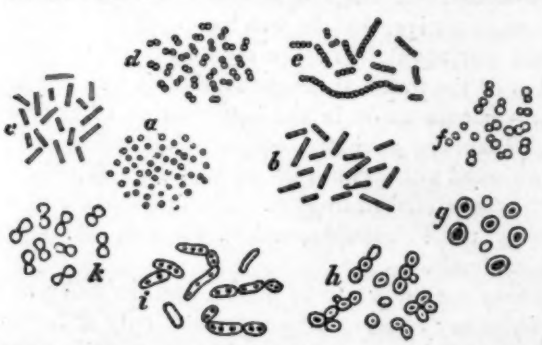


FIG. 1.—*a*, Micrococci; *d*, Micrococci, multiplying by scission; *b*, *c*, Bacteria (rod Bacteria, B. termo; *e*, vibriones; *f*, *h*, *i*, *k*, Torulæ, or Yeast-Plant, sprouting; *g*, probably "Dauersporen," durable or resting spores.

cal bodies, round, oval, or rod-like and jointed, varying in length from five ten-thousandths to one one-hundredth of a millimetre, and found especially in putrefying vegetable and animal infusions. The term *micrococcus* has always been restricted to the small, round, or ovoid bodies, but *bacterium* and *vibrio* have been applied indiscriminately to all, the former being more commonly used in France and Germany, the latter in England. When used in the narrower sense, *bacteria* denotes stiff, rod-like bodies, single or jointed, motionless or endowed with an oscillatory movement in place, while *vibrio* is applied to those which move rapidly across the field of the microscope with an undula-

tory, sinuous motion. Another variety, less frequently found, is larger, and has the form of a corkscrew, with from one and one-half to three or even four complete turns. These spiral forms are perfectly rigid, and rotate about their long axis with great rapidity, moving rapidly forward and backward with the regularity, although not the deliberation, of a pendulum. Their rotation about the long axis gives rise to the appearance of a wavy, serpent-like motion, which has deceived many observers, and it is very probable that the smaller vibriones mentioned above owe their apparent sinuous motion to the same cause. The ease with which one may be deceived on such a point will be readily understood by any one who has noticed a large screw in motion, or the shadow of a slowly-turning corkscrew.

Leeuwenhoek, a Dutchman, published, in 1684, the earliest observations of bacteria of which we have any record. He found them chiefly in the matter picked from between his own teeth and those of his acquaintances, and, animated apparently by the same spirit for which his countrywomen are so noted, he defends himself against a possible charge of uncleanliness by mentioning that he habitually brushed his teeth after every meal; but he also records that he found the largest quantity of bacteria between the uncared-for, broken teeth of an old man. He supposed these bacteria to be animals, and, indeed, gave the name *eels* to some large ones which he found in vinegar, whose motions were so active that he was "obliged to kill one before the limner could portray it." In the eighteenth century Müller made a classification of the forms then known, but it was not until after the great improvements made in the construction of microscopes, about 1820, that Ehrenberg gave the complete description and classification which have served almost until the present day. His book ("Infusionsthierchen") was published in 1838. The different forms were grouped in one family, the *Vibrionidæ*, and, as the title shows, were still supposed to be animals.

To-day they are known to be plants, and the different varieties are supposed by many observers to represent only different periods of development. Robin asserted several years ago that the ordinary rod bacteria could develop into the long thread bacteria, and even into the long filaments of leptothrix found so constantly in the mouth; but this was not generally believed, and the latest complete classification, that of Ferd. Cohn, published in 1872, is based upon the absence of such a developmental relationship. The study of these plants is rendered very difficult by their extreme smallness, and all attempts to cultivate them under the microscope, in "wet chambers," have failed to disclose the secrets of their growth, on account of the abnormal conditions in which they are necessarily placed. A considerable depth of liquid seems to be essential, as do also the presence of air, and protection against shocks or jars, and movement of the liquid; consequently, they can be studied only by comparing the forms found at intervals during

a long period of time, either when cultivated artificially, or when growing spontaneously under natural conditions.

In this way Billroth, who has published the most recent and, in many respects, the most remarkable work upon the subject, was able to make out the whole series of changes, from the spore to the long filaments, by using for his observations the yellowish mould which formed on the wall of his laboratory, where the water leaked slowly from a loose faucet. At the same time he discovered the nature and importance of certain glistening spherical bodies frequently found in infusions containing bacteria, and called *Dauersporen*, or durable spores (Fig. 1, *g*), by Cohn, although he did not think bacteria were developed from them. Billroth demonstrated that these *Dauersporen*

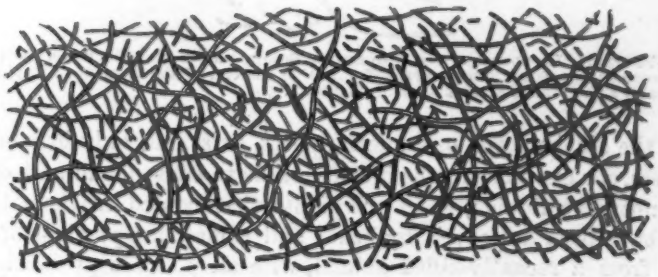


Fig. 2.—Bacteria and Leptothrix Filaments found in a Turnip-Infusion.

form micrococci in their interior, which are set free by the bursting of the envelope, and are then capable of multiplication by scission, or of lengthening into bacteria; also, that they are endowed with great vitality, and are not destroyed by freezing, boiling, or drying. He had some which germinated after they had been kept dry for eight years; and, whenever he wished to make sure of the destruction of the spores contained in his experimental liquids, he heated them to 392° Fahr.

They are formed in the interior of bacteria, and sink to the bottom of the liquid which contains them. The importance of these facts in their bearing upon the question of spontaneous generation, and upon the innumerable repetitions and variations of the experiments with sealed flasks, which have attracted so much attention since Pasteur made them, cannot be over-estimated; for, as Prof. Wyman, of Cambridge, says, "The issue between the advocates and opponents of the doctrine in question" (spontaneous generation) "clearly turns on the extent to which it can be proved that living beings resist the action of water at a high temperature."—*American Journal of Science and Art, September*, 1867.

Bacteria themselves are much more easily affected by heat and cold than are these *Dauersporen*. Their motions cease when the temperature is reduced nearly to the freezing-point; but it may be

carried even below zero, and yet the movements will recommence as soon as it is raised again above 40° or 45°. Exposure to a temperature of 140° will kill them; but this result seems to depend quite as much upon the length of time during which they are exposed to it, as upon the degree of heat itself, several hours being required for the lesser degrees, while ten to fifteen minutes, at boiling heat, are sufficient, and even four or five at 215°.

FIG. 3.—BACTERIA, LEPTOTHRIX, AND SPORE-LIKE BODIES FOUND IN A SOLUTION OF AMMONIC CARBONATE AND SODIC PHOSPHATE.—The tuft at the end of the filament of Leptothrix is probably an accidental accumulation of fragments.

In the air, bacteria, or bacteria-spores, exist, but only in moderate numbers, for exposure to the air often fails to cause cloudiness of artificial nutritive liquids, when the plants that existed in them beforehand have been destroyed by heat; and sometimes portions of meat taken from a recently-killed animal, with all possible precautions to prevent inoculation with bacteria through the instruments employed, and placed in open vases that have been washed in alcohol and then scorched in a hot flame, remain for days and weeks without putrefying. They are present in all kinds of water, and generally in considerable numbers. Cohn found them in the vapor condensed upon the inner surface of a bell-glass placed over a dish of water; and it is probable that those found in the air are enabled to live by the moisture contained in it. Their presence in the liquids and tissues of the body, often affirmed and denied, is now proved beyond question, Billroth's experiments on this point having been repeated and confirmed very recently by Tiegel. Rapid multiplication in the living body is prevented in part by the motion of the blood, and in part by the vital energy of the tissues, which is so vigorous that these plants cannot check it, and thereby obtain the nourishment needed for their own growth; but, when life has ceased, or when an abnormal condition of the tissues has been brought about by any cause, then rapid growth begins, and we have, in the one case, putrefaction; in the other, various pathological changes of more or less importance.

About the year 1865, two physicians of Strasburg, Messrs. Coze and Feltz, published a series of experiments which they had made with inoculations of putrid matter; and in 1872 they published a book upon the same subject, claiming that the virulent effects of putrid matter were due to the presence and growth of bacteria, and that the blood of an animal poisoned with such matter was itself virulent to a

high degree, and that this virulence was increased by successive inoculations. A similar series of investigations had led another French physician, Davaine, to the opinion that the disease called anthrax, when it occurs in animals, and malignant pustule when it affects man, was caused by a certain variety of bacteria, to which he gave the name *bacteridia*.

Davaine repeated the experiments of Coze and Feltz, and in September, 1872, read before the *Académie des Sciences*, in Paris, a report of three series of inoculations with putrid blood, the results of which were so startling that for several months the discussions in the Academy turned almost exclusively upon the subject of septicæmia, or blood-poisoning. The first series showed that inoculation of a rabbit with a drop of blood, putrefied in the open air, rarely killed the animal, and that sometimes ten or fifteen drops were necessary. The second series comprised successive inoculations of blood from one septicæmic animal to the next, and showed that $\frac{1}{10}$ to $\frac{1}{100}$ of a drop was sufficient to kill the fifth, $\frac{1}{10000}$ to $\frac{1}{20000}$ would kill the tenth, while, for the twenty-fifth, the one-ten-trillionth part of a drop was fatal.

Incredible as some of these assertions seemed, they were verified by many experimenters; but the minimum dose that would certainly kill was placed at the one-millionth part of a drop. Davaine claimed that the active poisonous principle was the bacterium, which, by its growth and multiplication in the blood, acted as a ferment; and this opinion, supported by Pasteur, was generally accepted, and it was supposed that the ordinary acute inflammatory complications of wounds, accompanied by symptoms of general poisoning, were caused by the accidental entry of bacteria. The same opinion had been held before, and the novelty of Davaine's views lay chiefly in the excessive minuteness of the quantity necessary to produce the effect.

The chief benefit derived from these experiments and discussions in Paris was found in the great interest which was excited everywhere in the question. The experiments were repeated, and the conclusions examined in almost every pathological laboratory in Europe, and we have every reason to expect that, through this general examination and discussion, the truth will appear. From time to time articles appeared denying the virulence claimed for bacteria; the earliest of these was a paper submitted to the *Académie des Sciences*, in April, 1873, by M. Onimus, who had been experimenting under the direction of Prof. Robin. He placed putrefying blood in a bag made of a dialytic membrane, and immersed the whole in distilled water, which, after a few hours, was found to be filled with bacteria. Inoculation with the blood produced the usual results, but inoculation with the water caused no septic symptoms whatever; on the other hand, the same blood, when subjected to various processes which removed or destroyed the bacteria, retained its virulence, and from these ex-

periments he drew the conclusion that the virus of putrid infection is not an organized ferment, not bacteria, but an albuminoid substance. Panum, who held similar views twenty years ago, has reasserted his belief in them in an article published in *Virchow's Archives*, for July, 1874, and during the last year the weight of testimony has all been in this direction. Clinical observation has been employed to confirm or refute the conclusions of experimental pathology, and has clearly demonstrated that the poisonous processes, of which we have spoken, can begin in the human body and proceed without the presence of bacteria, and that bacteria may be present in large quantities without the slightest symptom of any poisonous complication.

FIG. 4.—TORULÆ.—From a Solution of Ammonia Tartrate and Sodic Phosphate.

It is probable that their *rôle*, so far as disease is concerned, is as follows: While they have no power in themselves to excite disease (*diphtheria*, *vaccinia*, *septicæmia*, *typhoid fever*, etc.), they are able to absorb the poison (ferment?) which is capable of producing it, to "fix" it, as it is termed, and to give it up to any tissue with which they may come into contact, acting thus as carriers of contagion; then, after the abnormal process has been commenced in the body, a change is brought about in the tissues which renders them suitable for the rapid growth and multiplication of the bacteria, which, in turn, augment the change in the tissues, and thus there is formed a vicious circle, the consequences of which are too often fatal.

Any agent which destroys the life of the bacteria, or prevents their multiplication, breaks this circle and renders a cure possible.

The influence of bacteria in fermentation is still undecided, but, for the sake of completeness, the different opinions should be mentioned. It is admitted by all chemists that these or similar organisms are invariably present in some fermentations—*Bacterium termo*, or rod bacterium, in putrefaction, and the yeast-plant in alcoholic fermentation, for example; and while some claim that the process is due to the vitality and growth of these plants, others hold that this growth is an effect, not a cause; and a third party claim that the plants secrete the actual ferment.

According to Liebig, fermentation is an action which is produced in a fermentable substance by an albuminous matter which is dead and in spontaneous decomposition; that is, fermentation is a correlative phenomenon of death. On the other hand, Pasteur maintains

that fermentation occurs only when a microscopical vegetable organism nourishes itself and multiplies at the expense of a part of the fermentable substance. All fermentation is accompanied by life; both processes (fermentation and growth) begin and end simultaneously; that is, fermentation is a correlative phenomenon of life.

The question of spontaneous generation is closely bound up with the question of fermentation, the supporters of the former claiming that life has its origin in a new combination of elements—*archebiosis*, a combination which resembles that which occurs in crystallization, and that the same causes which induce the chemical decompositions and subsequent combinations known as fermentation also bring about the combinations which result in life. The discussion has been very active, and, in France, Pasteur, skillful dialectician as he is, had to fight long and hard for the victory which he won. History shows that spontaneous generation has often been invoked to explain an unknown mode of production of life, and has always disappeared before advancing knowledge, and, now that it has been overthrown when claimed for the lowest forms of microscopical life, it probably will not reappear until an advance in the means of observation shall have revealed to observers still lower and more minute forms.

ANIMALS NOT AUTOMATA.

By ROWLAND G. HAZARD, Esq.

THE doctrine of necessity has been ably advocated by many acute philosophers, and is to-day, in various forms, including fatalism, the accepted creed of a large portion of mankind. A doctrine thus supported, and so immediately bearing upon our actions and our powers, cannot but be worthy of serious attention.

Prof. Huxley, approaching it on the material side, in the true spirit of philosophical inquiry, trustingly following wherever truth seemed to him to lead, and regardless of the apprehended consequences of attacking dominant creeds and opinions, has pushed this doctrine to its legitimate logical consequences, in the conclusion that all animals, man included, are but "conscious automata," moved and directed in their movements by extrinsic forces.

With him, I believe that all progress in knowledge is beneficial; I deprecate no enterprise in experiment, nor any boldness in speculation, if we are duly cautious in accepting and applying its results. The revelations of intelligent and honest inquiry always merit respectful and careful consideration, but are not properly exempt from scrutiny.

Although I have perhaps deviated as far on one side of the current opinions as Prof. Huxley has on the other, I cannot claim any credit

for fearlessness of the consequences—my only apprehension in that respect being, that any arguments I may present, unrelieved by interesting experiments, will not excite sufficient interest to provoke either commendation or censure.

I think, however, I may properly say that, viewing the problem on the spiritual side, and carefully excluding popular prepossessions and theological dogmas, I have carried the opposite doctrine of "freedom" to its legitimate logical consequences in the conclusion that every being that wills is a creative first cause, having, in virtue of its attributes of knowledge, feeling, and volition, a power of itself to begin action. That the object of every volition or effort is to make the future different from what it otherwise would be, and hence, that every such being is an independent, self-active power in the universe, freely doing its part and coöperating with all other active intelligences in creating the future, which is always the composite result of the action of all such intelligences: that even an oyster, though it have no other power than that of moving its shell, may, so far, create the future and make it different from what it otherwise would be; and further, that as every intelligent being will conform its action to the conditions under, or upon, which it is to act, the action of each, in changing the conditions, may affect the action of any or of all others, and the action of the lowest may, in this way, influence that of the highest.

We both, however, admit knowledge and feeling, and recognize consciousness, or the phenomena of knowing, in man and other animals. In discussing questions so fundamental, this must be largely relied upon for the foundation and support of the argument on either side, and I will briefly state my views in regard to its authority.

Mind, as manifested in man and in brutes, I regard as entirely made up of a *capacity* for knowledge, a *susceptibility* to feeling, and a *faculty* of effort (will); this last being the only *power* we possess; and if it—the effort of intelligent being—is not the only power *known* to us, it is at least that power, of the existence of which we have the most direct and reliable evidence. The recipient and receptacle of all our knowledge, whatever its source, is consciousness. Our conscious perceptions and feelings (including emotions) are the foundation of all knowledge, and all belief; but the consciousness of one man, of itself, avails nothing against another having a different consciousness and a different belief. Belief is not a matter of will or of choice, but each must believe in conformity to his own consciousness, and retain his existing belief till his consciousness is in some way changed. The denial of this involves a contradiction, and we may assume, as a corollary to it, that it is not only reasonable, but a necessity, that we believe things to be as they appear to be, till we recognize a sufficient reason for believing that the appearances are deceptive. The testimony of consciousness is not equally reliable as to all subjects. In some cases it is conclusive, in others far from it. In regard to our internal percep-

tions, sensations, and emotions, our consciousness is conclusive evidence that we have them, and that they are what consciousness represents them to be. The consciousness of the sensation of pain is the pain itself; and the consciousness of perceiving that the whole is greater than its part is, itself, the perception of that fact, and there can be no question as to my actually having the sensation of pain, or as to my having the perception of the inequality. But the consciousness is not conclusive as to the conformity of the perception with the existing fact, nor as to any inference which I may draw from the sensation. One may have as full and decided perception of what is not, as of what is; and the liability to erroneous inferences from our sensations is a matter of daily experience.

Even a universal belief, founded on entire uniformity in the perceptions, or in the inferences from our sensations, is not conclusive. If it were, no error in such belief could ever be corrected. If, for instance, the belief that the sun daily revolved around the earth was once universal—and universal belief is regarded as conclusive—the present belief never could have been substituted. Still, to assume things to be as they appear to be, till a sufficient reason is given to the contrary, is a necessary condition to our progress in science and philosophy. If this proposition is denied, then all Prof. Huxley's array of facts and arguments may be fairly met by saying, "True, these things appear to be as you say, but, then, this appearing is no reason for supposing that they really are so." There would be an end to at least all physical investigations.

Instinct is, perhaps, a more important element in this discussion than Prof. Huxley has suggested—the various and vague notions in regard to it which obtain both in the popular and philosophical mind do much to confuse the consideration of voluntary and mechanical action.

Prof. Huxley assumes that instinctive action is mechanical. He says: "When we talk of the lower animals being provided with instinct, and not with reason, what we really mean is that, although they are sensitive, and although they are conscious, yet they act mechanically, and that their different states of consciousness, their sensations, their thoughts (if they have any), their volitions (if they have any), are the products and consequences of their mechanical arrangements. I must confess that this popular view is, to my mind, the only one which can be scientifically adopted."

There is much and high authority for the doctrine that instinctive actions are mechanical, but I believe it is very generally rejected by those who have observed the actions of animals without any knowledge of subtile theories to account for them. "What," incredulously exclaimed one of my grandchildren, on hearing of "Prof. Huxley's statement," "what sort of a mechanism is it that carries the wild-geese from Canada to the Gulf of Mexico every fall, and brings them back

every spring?" This seems to me a fair illustration of the prevailing notions, indicating that Prof. Huxley is mistaken in assuming that the "popular view" favors the mechanical theory. I am aware that the "popular view" cannot be urged against special inquirers, whose object often is to correct prevailing errors, as well as to extend the limits of our knowledge; but misapprehension of the popular view may give a wrong direction to their efforts and make them unavailing.

No mechanical contrivance, no mechanism furnishes any *power*, but is only a means of applying power; and, even if the term mechanical embraces all the phenomena of matter in motion, we still have the question as to whether the mechanism or the matter moves by its own power, or is moved by the effort of some intelligent being.

He who views the perfect crystal as the direct creation of an intelligent designing power, and he who sees in it only the orderly effect of natural forces, will alike class it with the mechanical; so, too, both would speak of the celestial mechanism.

In investigating the *laws of Nature*, the one is observing and generalizing the uniform mode of God's voluntary action, the other is finding the necessary consequences of the action of material forces. Each attributes any phenomena which he cannot class with any of his generalizations to some inscrutable exercise of power—the one to intelligent effort, the other to unintelligent material movements; so that, in the mechanical, we have still the question as to the two forms of power—intelligence in effort, and matter in motion; and, as between these, admitting the existence of both, it seems most reasonable to attribute instinctive action, the action of a conscious, and hence intelligent being to the former, rather than to the latter. Instinctive action is not mechanical, even in the most extended sense of the term, but must be referred to the power of the being itself, and not to extrinsic power of any kind. Every voluntary effort is put forth to gratify a want, to make the future in some respect different from what, but for the effort, it would be. To do this, always requires that the effort, or series of efforts, should be adapted to the specific object, and that, in any series of them, each one should be in the appropriate consecutive order. There must be a mode, or plan of action. This plan is either a part of our knowledge, or is formed by means of it.

In all our actions, whether instinctive, rational, or habitual, we thus apply our knowledge to direct our efforts to the end desired, and there is not in the actions themselves, nor in their immediate antecedents, any difference whatever. In all of them it is but an effort suggested by the want, and directed to a given end by means of our knowledge. The difference is not in the action, nor in the knowledge, nor in the application of the knowledge, but one step farther back—in the manner in which we became possessed of the knowledge we apply. In a rational action we, by a preliminary effort, obtain this knowledge—we make the requisite plan. In the instinctive action this knowledge is

innate; the plan is ready formed in the mind, requiring no premeditation, no deliberation to determine the mode of action. In the rational actions we acquire the knowledge of these plans for ourselves, and it is the preliminary effort to determine what to do, and how to do it—to find the mode of action—that tasks our intellectual abilities. But, when we have once formed the plan, and acted upon it often enough to remember its successive steps, so that we can repeat them in action by rote without any reference to the *rationale*, it becomes a plan ready formed in the mind, and the acting upon it becomes habitual. The instinctive and habitual actions, then, are precisely alike in this, that both are in conformity to a plan ready formed in the mind, requiring no effort to form them for the occasion, and differ only in this, that in the instinctive we found the plan ready formed, while in the habitual we *originally* formed it by our own effort. If, after the latter plans had become fixed in our memory, we should forget that we had originally acquired them by our own effort, we would know no difference between the instinctive and habitual action.

The popular consciousness of this similarity is expressed in the common adage that habit is second nature. If this view, which seems to me to account for all the peculiarities of instinctive action, is correct, instinct is not a distinct faculty, capacity, property, or quality, of being, which may be compared with or substituted for reason, but has relation only to the mode in which the knowledge by which we determine some of our actions was originally obtained. Whether the innate knowledge of modes and plans is by transmission, or otherwise, does not affect the theory. It is sufficient that they are thus ready formed in the being without effort of its own.

All intelligent actions, except perhaps those which are merely imitative, must in the first instance be either instinctive or rational, the habitual coming later through the transformation of the others by repetition and memory; the instinctive, however, not being materially changed thereby.

But the foundation of all our actions must be instinctive, there being no possible way in which we could ever learn that effort is the means of using either our muscular or mental powers.

In regard to the rational actions, I see no distinction in kind, but only in degree, between those of man and the lower animals. Descending in the scale of intelligence, we may, and probably will, reach a grade of beings which do not invent or form plans to meet new occasions for action, and the efforts of such must be wholly instinctive; but I have seen both dogs and horses draw inferences, and work out ingenious plans of action, adapted to conditions so unusual and so improbable to them, as to preclude the assumption that they had been specially provided by Nature, through hereditary transmission, or otherwise, with the knowledge of the plan suited to the occasion.

Prof. Huxley asserts that matter is a cause, a power not only in

what is generally regarded as its own sphere, but that it also produces all mental phenomena. At the same time, while admitting the consciousness—the intelligence—of man and brutes, he denies to them the faculty of will, thus virtually denying to them any power.

He thus raises the question as to the power of matter, and also as to that of intelligent beings; at least of beings of no higher grade than man. It is not very clear whether or not he denies *all* intelligent power. In saying he has with him "Père Malebranche, who saw all things in God," he seems to recognize a supreme power; but then this power in his system might logically be but a deification of material forces, ignoring intelligent activity.

Against attributing power to matter, we may urge that its existence as a distinct entity has never been proved, and is seriously questioned. To assume that so important a quality inheres, and especially to assume that it inheres only in something, the existence of which is doubtful, when it may, with equal reason, be attributed to something, the existence of which is admitted, would be a grave philosophical and logical mistake.

Prof. Huxley admits the existence of intelligent (conscious) beings, but perhaps does not admit that power may, with equal reason, be attributed to them, nor perhaps that there is any reasonable doubt as to the existence of matter as a distinct entity; leaving these two questions open to discussion. In regard to the latter, he will probably admit that there is no decisive proof, and that the existence of matter is only an inference from the sensations which we attribute to its agency. But all the phenomena of these sensations are as well accounted for on the hypothesis that they are directly produced in our minds by some intelligent power as that they are the effects of matter.

If the material universe is regarded as the work of an intelligent Creator, working with design to produce a certain effect, then, upon either hypothesis, it is the expression of a conception of this Creator, existing as thought and imagery in his mind before he gave it palpable, tangible existence in ours, and the only question between the two modes is, whether, in making it palpable to us, he transfers this thought and imagery directly to our minds, or first paints, moulds, or carves them in a distinct material substance. The external universe would not, in the first of these modes, be any the less real. The sensations, which are all that under either hypothesis concern us, or that we know any thing about, would be the same in both cases. But we can no more impute power to such imagery than to an image in a mirror, and under this hypothesis material sensation would have no existence.

One consideration favoring the ideal theory is, that, under it, creation becomes more conceivable to us. We can, any of us, conceive or imagine a landscape, and vary its features at will. This is an incipient creation which, if we could impress it upon the mind of another, would

be to him an external creation—to his vision as thoroughly material as the fields, and streams, and trees, he now looks out upon; and, if from any cause it should become fixed in the mind of him that conceived it, so that he could not change it at will, it would become to him an external reality. And this sometimes happens in abnormal conditions of the mind. In order to thus create what, at least to the visual sense, would be an external material creation, the only addition, then, which is required to the powers which we habitually exercise is that of impressing our conceptions upon others. With this addition we could create and give palpable existence to a universe, varying more or less from that now palpable to us. And this power of impressing our conceptions on others we are none of us wholly devoid of. Sculptors, painters, architects, and more especially poets, have it in marked degree.

We, however, find no rudiment of force in these incipient creations of our own, and, hence, they furnish us with no logical ground for attributing it to similar and more perfect creations of a Superior Intelligence. That these creations of our own are mostly evanescent, and those to which, with great labor, we give a persistent reality are very limited and imperfect, does not disprove the position that creation is more conceivable to us upon the ideal hypothesis than upon the material. The ideal hypothesis is also commended by the consideration that man, having, in a finite degree, all the other powers usually attributed to the Supreme Intelligence, lacks, under the material theory, the power of creating matter. Corresponding to His omnipotence, omniscience, and omnipresence, man has finite power and finite knowledge, and can make all the objects of his knowledge present, which is equivalent to a finite presence, limited, like our other attributes, to the sphere of our knowledge. This hypothesis, then, rounds out our ideas of creative intelligence, relieving us of the anomaly of the creation of matter as a distinct entity, for which, having in ourselves no conscious rudiment of a power to accomplish, we cannot conceive the possibility.

I may further observe that, if I am right in supposing that the only difference between our own incipient creations, of a landscape for instance, and the external scenery which we perceive, is that we can change the former at will, while the latter is fixed, it shows how narrow is the space that divides the creative powers of man from those of the Supreme Intelligence, and that the difference is mainly, if not entirely, in degree, and not in kind. This gives warrant to the logic, and shows how short the steps by which we attribute all creations and all changes, which we regard as beyond our own power and beyond that of other embodied intelligences known to us, to a superior intelligence, with the same powers which we possess and use to create and change, increased, I will not say infinitely, but to a degree corresponding to the effects which we see and ascribe to them.

If the existence of matter be admitted, it may still be urged that,

being unintelligent, it can have no causative power, and can produce no change, for all changes in matter must be, by its motion, massive or atomic, and matter cannot move itself.

Even if it could be imbued with motive power, it could have no inducement, no tendency, to move in one direction rather than another; and a tendency which is equal in all directions is no tendency in any direction. If all matter were at this moment quiescent, even the materialists will not assert that it could of itself begin to move.

It may, however, be urged that both the arguments thus drawn from the difficulty of conceiving the creation of matter, and the necessity of motion to its causal power, may be met by the hypothesis that matter was not created, but has existed through a past eternity, and that its original condition was that of motion, and that there is no more difficulty in conceiving this than in conceiving that intelligence, with its activities, has had no beginning.

But, granting that matter has always existed, and originally had motion, and consequent power, still, if the tendency is to expend and exhaust this power in producing effects, by collision or otherwise, or, admitting the conservation of force, if its tendency is to become merely potential, then the force which it originally had, in virtue of being in motion, must, in the infinite period of its existence, have been either wholly exhausted or reduced to an infinitesimal, requiring the intervention of some active power to again give it any practical force.

But whether matter, supposing it to exist, can of itself, by means of its motion, be an independent power or force, still depends on another question, viz., Is the tendency of a body in motion, when the power which put it in motion is withdrawn, to continue to move, or to stop? In other words, is the application of extrinsic power required to keep it in motion, or is such application required to stop it? Having no power to move itself when once at rest, it could have no power to act, but could only be acted upon, and, if it has inertia, it would be a means of exhausting other force.

If when once in motion its tendency is to continue in motion, then it could be used as an instrument by which intelligent power, putting it in motion, could extend the effects of its own action in time and space.

If the tendency is to stop, then it could have no power or force, in virtue of being in motion, and could not even be a means of extending the effects of the action of other powers.

I have heretofore confessed my inability to solve this question as to the tendency of a moving body to continue its motion, or to stop when the motive power is withdrawn. I have not, perhaps, been able even to disentangle it from the empirical meshes in which it has become involved, and which, in my view, do not and cannot furnish any clew to its solution; but, until this point is settled, I do not see how matter, though in motion, can properly be regarded as a force, or even as a conserver of force imparted to it by some other power.

If matter in motion is power, then all its effects must be such as take place of necessity, it having no power to select or vary them, and, whatever the course of such effects, it cannot change. If, for instance, the moving body is approaching another body, then, as two bodies cannot occupy the same space, some effect must of necessity result from the collision; and all the effects of unintelligent cause or force must be from some like necessity. In this case the material hypothesis has an advantage, there being no apparent connection of *necessity* between an intelligent effort and its sequences. This, however, as matter cannot put itself in motion, nor, perhaps, even continue any motion imparted to it, may only make it an instrument of other power, and not a power itself.

Some of the considerations in favor of the existence of intelligent power have already incidentally arisen in connection with the question of the existence of material force, and others pertain to that of the will, to which we will now turn.

The question which Prof. Huxley raises is not merely, Does man will *freely?* but, Does he will *at all?* If he recognizes any volition in us, it is a volition in which we have no agency, but of which we are only conscious.

Between the two questions, of willing freely or not willing at all, there is perhaps little of practical importance; for, if our actions are controlled by some extrinsic power or force, it is not important whether this control is exerted directly on or in the action, or indirectly through controlled will. It might, perhaps, even be properly urged that, philosophically as well as popularly, a willing which is not free is a willing which is not willing, and this would identify the two questions.

Prof. Huxley, from divers physical experiments, comes to the conclusion that animals, including man, do not will, but that the effort-phenomena, of which we are conscious, are only a series, or the effect of a series, of mechanical changes of matter, over which we have no control. He admits that we have knowledge and feeling, and there is no difficulty in conceiving that these may exist without will, though the existence of either feeling or will without knowledge is impossible.

To most persons the actual making of an effort, or willing, seems to be as fully attested by their consciousness as a sensation is; and there is high philosophical authority for putting it in that category, in regard to which the consciousness is positively and of necessity conclusive. It seems to me, however, that there is room for a distinction between the consciousness of effort, and the effort itself. If the changes, which seem to us to be the consequences of our effort put forth with a preconception of these changes, and for the purpose of producing them, are really caused by some extrinsic power or force acting through us, it is quite conceivable that such a power, especially if intelligent, may impress us with the emotion of making an effort

when we make none, though I see no reason why such a circuitous mode of action should be adopted. But, though the consciousness of making an effort is not conclusive as to the *actual* making, still, as it is of internal phenomena, it is evidence of a higher order than that which consciousness of a sensation gives as to the existence or character of the external phenomena.

The senses through which the external is presented may not act perfectly; and this, as compared with the consciousness of internal phenomena, makes an additional risk of error similar to that which arises from seeing an object through glass or in the reflection of a mirror, instead of directly without any intervening medium.

Those, then, who set up physical phenomena against our consciousness of effort, labor under the disadvantage of impeaching the accuracy of the testimony by other testimony which is less reliable than that which they impeach.

Prof. Huxley admits that men and other animals know and feel. The existence, then, of that for which *power by effort* is claimed as an attribute, with these prerequisites to its exercise, is admitted.

On the other hand, any belief in matter or in its motion is but an inference from our sensations which, as we have seen, is not a necessary or conclusive inference; and hence we have no reliable evidence of the existence of matter, nor of the attributes which, if it exists, are essential to its having power.

In the first case, we know the existence of the active agent; its feeling, subjecting it to want; and its knowledge, enabling it to adopt a mode of gratifying its want, which are all the elements which are requisite to the exercise of a power by effort, and though we have no conclusive proof that it actually makes the effort, the testimony in regard to this, for reasons already stated, is more reliable than the inferences from our sensations, that matter exists, and that it moves, and that one portion impinges on another portion, all of which are essential to material causation. In the first case, the existence of the agent, with all the prerequisites to the exercise of power, is known. In the latter, not a single one of them is known. This shows that the material phenomena which Prof. Huxley presents are not, in this case, sufficient to rebut the testimony of consciousness that we do will—do make effort, and thereby produce change.

The further question, Do we ourselves determine our efforts? is identical with that of our freedom in willing, which I do not propose here to discuss, but will remark that it is not probable, perhaps it is not conceivable, that any unintelligent agent should create the whole system of wants, knowledge, and the application of knowledge involved in an effort, as just stated, and impress the *whole* as illusions on the mind of the actor; nor yet, that any blind force should direct the effort in exact conformity to the wishes and the preconceptions of the manner and the effect which are in the thoughts of him who has

the emotion of making an effort, and which the unintelligent power, or agent, of course cannot know. Only an intelligent agent could know this; and, if the conforming of the effort to this want, knowledge, and preconception of the effect, must be referred to some intelligent being, it seems most reasonable to refer it to that which directly feels its own want, knows its own perceptions of the mode of gratifying the want, and its preconceptions of the effect to be produced, to all which the effort is to be conformed, and which, at the same time, is conscious of making the effort, and of thus conforming and directing it by its own knowledge. Between the sensation of making the effort, and the antecedent and subsequent knowledge of the subject of this sensation, there is a harmony which it seems hardly conceivable should be produced by any power not having this particular knowledge, and much less by a power incapable of knowing any thing.

As germane to the whole question of intelligent and material power, I will suggest that it would be unphilosophical to assume the existence of two primary powers, when one is sufficient to account for all the phenomena, and that as it seems hardly conceivable that matter should create intelligence with its phenomena—that what does not know should create a power to know—while, as already shown, it is quite conceivable that intelligence should create all that we know of matter and its phenomena, the hypothesis of power in matter should, on this ground, be discarded.

Let us now look at the very curious and interesting experiments upon which Prof. Huxley relies for his conclusion that animals, including man, are "conscious automata." He says that, if, when a man is so paralyzed that he is wholly unable to move his limbs, and has no sensation in them, "you tickle the soles of his feet with a feather, the limbs will be drawn up just as vigorously, perhaps a little more vigorously, than when he was in full possession of the consciousness of what happened to him." He also states that, in the case of a frog similarly paralyzed, the result of irritating the skin of the foot is the same: in both cases the foot being drawn *from* the source of irritation. This certainly bears a very close resemblance to the voluntary action of an intelligent being, conscious of the irritation, and seeking relief from it by its own efforts. Prof. Huxley, however, positively asserts that the animal could not feel or will, and this being so, he seems to be justified, by common usage, in calling the action "mechanical." But, as I have already suggested, this term is applied to material phenomena, whether they are results of matter in motion, or of the uniform modes of God's action.

Other experiments still more remarkable are presented. He says: "Take this creature (the same frog), which certainly cannot feel, and touch the skin of the side of its body with a little acetic acid, which, in a frog that could feel, would give rise to great pain. In this case there can be no pain. . . . Nevertheless, the frog lifts up the limb on

the same side, and applies the foot to rubbing off the acetic acid; and what is still more remarkable, if you hold down the limb, so that the frog cannot use it, he will, by-and-by, take the limb of the other side and turn it across the body, and use it for the same rubbing process."

This goes a step further, requiring a more complicated mechanism to direct the force, when it fails to move one foot, to the movement of the other. In still another case, he says: "Suppose the foremost two-thirds of the brain taken away, the frog is then absolutely devoid of any spontaneity; it will remain forever where you leave it; it will not stir, unless it is touched; . . . but, . . . if you throw it in the water, it begins to swim—swims just as well as the perfect frog does; . . . and the only way we can account for this is, that the impression made on the sensory nerves of the skin of the frog by the contact of the water conveys to the central nervous apparatus a *stimulus* which sets *going a certain machinery* by which all the muscles of swimming are brought into play in due order of succession. Moreover, if the frog be stimulated, be touched by some irritating body, although we are quite certain it cannot feel, it jumps or walks as well as the complete frog can do."

Most persons, I presume, have seen men and other animals made so torpid by injury or disease, that they would show little sign of vitality, and great indisposition to make any effort, but that they still moved when pricked with a pin has been generally regarded as evidence that they still felt; and the movements they would make to avoid danger, or escape pain, have been thought to be conclusive that they were *not* "absolutely devoid of any spontaneity."

It is not uncommon for a man, who, in ordinary circumstances seemed wholly unable to move his limbs, under great or sudden excitement, as the approach of fire or sudden apprehension of drowning, to make vigorous and successful muscular efforts.

The common observer, then, would infer from the foregoing experiments that Prof. Huxley was not justified in inferring, from the fact of mutilation, that the frog was "absolutely devoid of any spontaneity," and that "we are quite certain it cannot feel." If the facts stated do not prove that the frog still feels, still wills, and still has knowledge to direct its efforts to get rid of the irritation, it seems difficult to devise any mode of proof that a being ever feels, knows, or wills. Prof. Huxley admits that we do feel and know, but infers from these experiments that we do not will. If his theory of them is correct, they seem to afford little ground for this distinction.

Prof. Huxley, in still another case, says of a frog deprived of the most anterior portion of the brain, that "it will sit forever in the same spot. It sees nothing, it hears nothing," yet placed on the hand would, on the turning of the hand, make all the movements necessary to prevent its falling off, and that "these movements are performed with the utmost steadiness and precision, and you may vary the posi-

tion of your hand, and the frog, so long as you are reasonably slow in your movements, will work backward and forward *like a clock*." Referring to this experiment, Prof. Huxley afterward says: "If the frog were a philosopher he might reason thus: 'I feel myself uncomfortable and slipping, and, feeling myself uncomfortable, I put my legs out to save myself, knowing that I shall tumble if I do not put them farther. I put them farther still, and my volition brings about all these beautiful adjustments which result in my sitting safely!' But, if the frog so reasoned, he would be entirely mistaken, for the frog does the thing just as well when he has no reason, no sensation, no possibility of thought of any kind. The only conclusion, then, at which there seems any good ground for arriving is, that animals are machines, but that they are conscious machines." And he afterward says: "Undoubtedly, I do hold that the view I have taken of the relations between the physical and mental faculties of brutes applies in its fullness and entirety to man." Of this last experiment Prof. Huxley further says: "And what is still more wonderful is, that if you put the frog on a table, and put a book between him and the light, and give him a little jog behind, he will jump (take a long jump, very possibly), but he won't jump against the book, he will jump to the right or to the left, but he will get out of the way, showing that, although he is absolutely insensible to ordinary impressions of light, there is still something which passes through the sensory nerve, acts upon the machinery of his nervous system, and causes it to adapt itself to the proper action." This is certainly very wonderful, and becomes even more so when taken in connection with the next case—that of a man who had been shot in the head, and who, Prof. Huxley says, "is in a condition absolutely parallel to that of the frog," but afterward says, "very nearly" in the same condition, and also says, "he has only one sense organ in a state of activity, namely, that of touch, which is exceedingly delicate." Yet of this man, thus described as virtually in the same condition as the frog, except that he has a very delicate sense of touch, we are told that, "if an obstacle is put in his way, he knocks against it, feels it, and goes to one side; if you push him in any direction, he goes straight on until something stops him."

It is certainly very remarkable that the frog, with no sense at all, avoids leaping against the obstruction, while the man, with a delicate sense of touch, and other conditions parallel or very nearly the same as the frog, knocks against it. It must be a very curious mechanism which can make such discrimination in the effects of its action.

Let us examine the case of the frog a little further. Prof. Huxley ascribes its leaping obliquely and not directly forward to "a something which passes through the sensory nerve, *acts upon the machinery* of his nervous system, and causes it to adapt itself to the proper action," and this "although he is absolutely insensible to ordinary impressions of light." Does Prof. Huxley mean that this "something"

passes through the book, and thus reaches the sensory nerve, and that, but for the intervening book, it would not pass that way? Under some circumstances, it might be that a conductor would facilitate the passage of a "something" which would not pass through the air, but in this case there is the difficulty of getting this "something" to the book, and then of sending it forward through the air. The only alternative seems to be to suppose that when there was no intervening book, a "something" passed to the frog which was necessary to cause it to jump directly forward, the passage of which the book prevented. Neither of these hypotheses seems satisfactory, even if no objection is made to the unknown "something."

To those skilled in scientific investigation it may not appear important, but I apprehend that many, like myself, not familiar with its modes, will regret that the experiment in this case was not pushed somewhat further. To find, for instance, what would be the effect when the obstruction extended equally to the right and to the left? What if it extended indefinitely both ways? And what, when it made an entire circle around the frog in the centre; and what if in different positions other than the centre.

But, even admitting, in all the cases, all that Prof. Huxley claims as ascertained facts, what does it all amount to further than that he has brought to light some additional phenomena which, like the movements of the material universe and the pulsations of the heart, must be referred to some inscrutable agency? He who believes only in intelligent power refers them, with all else that he does not effect by his own efforts, and which he regards as beyond the power of any known embodied intelligence, to a Superior Intelligence, acting through the instrumentality of matter or otherwise; while he who believes only in material causation attributes them to the influence of matter, in some form or some mode of its movement differing from those forms and modes which are familiar to him. Nor is it material how many steps there may be between the power applied and the effect. If there are three or thirty ivory balls in a right line, and the first of them is put in motion causing each one successively to impinge on the next, the final effect of motion in the last is caused by the power applied to the first. We may by our own efforts put the alleged power of matter in action, or may thus act through the uniform modes of God's action.

In voluntary muscular movement the intermediate effect of a flow of blood to the contracting muscle has long been known; now, the propagation of molecular movement is ascertained. That we are not conscious of the movement of the molecules indicates (though far from conclusively) that we do not ourselves move them, but this does not indicate that the muscular movement is not the result of our own effort working through other agencies. That he who throws the stone which kills a bird does not know what curve the stone will describe, nor by

what power its motion is continued after it leaves his hand, does not show that he is not the cause of the killing.

If the knowledge of the intermediate changes is a necessary condition to the exercise of the power which produces the final result, what becomes of the hypothesis of causation by material movements, or forces, which know nothing? In regard to the special phenomena in hand, it would seem that no power less facile, or less variable and adjustable in its application than that of intelligent effort, could be adequate; and that no blind power or force, the effects of which must of necessity be uniform, could, from the same conditions, produce such diverse effects as those attributed to the man and the frog.

Considering the clear line of demarcation which there is between those cases of change for which we are conscious of making effort and those for which we are not, I do not see how the discovery of any number of cases of the latter discredits the testimony of consciousness as to the former. All this exhibition of material phenomena, then, really weighs very little on either side of the question as to the existence of intelligent or material causality; and this little, I think, may be fairly claimed on the side of the intelligent.

There is another criterion which, as Prof. Huxley, in applying a somewhat analogous test, has very appropriately said, "though it could not be used in dealing with questions which are susceptible of demonstration, is well worthy of consideration in a case like the present." I cannot demonstrate, but I have great faith in the proposition that all progress in truth will increase the happiness and conduce to the elevation of man. I also have some faith in the converse of this proposition —that whatever tends to diminish our happiness and degrade our position will be found to be not true.

In this case, by adopting Prof. Huxley's views, we should be deprived of all the dignity of conscious power, and with it of all the cheering and elevating influences of the performance of duty; for that which has no power can have no duties. Instead of companionship with a Superior Intelligence, communicating his thoughts to us in the grandeur and beauty of the material universe—the poetic imagery, of which it is the pure and perfect type—and in his yet higher and more immediate manifestations in the soul, we should be doomed to an inglorious fellowship with insensate matter, and subjected to its blind forces. That sublime power—that grandeur of effort by which the gifted logician, with resistless demonstration, permeates and illuminates realms which it tasks the imagination to traverse; and that yet more God-like power by which the poet commands light to be, and light breaks through chaos upon his beautiful creations, would no more awaken our admiration, or incite us to lofty effort. We should be degraded from the high and responsible position of independent powers in the universe—co-workers with God in creating the future—to a condition of mere machines and instruments operated by "stimuli"

and "molecules;" and, though still with knowledge and sensibility to know and feel our degraded position—"so abject! yet alive"—with no power to apply our knowledge in effort to extricate, and to elevate ourselves. We might still have the knowledge of good and evil; but, having no power to foster the one, or to resist the other, this knowledge, with all its inestimable consequences—all the aspirations which it awakens, and all the incentives to noble deeds which it, in combination with effort, alone makes possible—would be lost. And with it, we might almost say, there would again be no death, for all mutation now being but changes in the indestructible atoms of matter, by means of its motion, also indestructible and eternal, there would be little left to die, as there would again be little left to live for. For all this, I see no compensation in the doctrines now so clearly and frankly presented.

CELESTIAL CHEMISTRY.[1]

By T. STERRY HUNT, LL. D., F. R. S.

AMONG the most significant advances in chemical theory are those relating to the action of heat on bodies. If we define chemistry, as I have been tempted to do, as that science which treats of the relations to one another of the different forms of mineral (i. e., unorganized) matter, and their transformations under the physical agencies of heat, light, and electricity, we shall see how difficult it is, in a sketch like this, to draw the line between physics and chemistry. This becomes still more evident when we see in light the chemical constitution of matter, as it were, revealed and made visible to us by the spectroscope, or study the electric current parting in a mysterious manner the components of bodies. Time would fail us to follow the trains of thought thus opened, but I cannot forbear to say somewhat of the relations of temperature to chemical species, and of the power of heat to unloose the bonds of chemical combination. The admirable researches of Grove, followed by those of Henri St.-Claire Deville and his fellow-laborers, have shown us that, at an elevated temperature, such bodies as water, hydrate of potassium, and hydrochloric acid, are more or less completely resolved into their constituent elements, the affinities of which are suspended. In the principle of dissociation by heat we have an explanation of many chemical reactions hitherto enigmatical. The decomposition of bodies by heat is, moreover, assimilated to the phenomenon of volatilization: the rate of decomposition at a given temperature varying with the pressure, and with the nature of the atmosphere which surrounds the unstable body. The phenom-

[1] Extract from Dr. Hunt's Address at the Northumberland Centennial, on "A Century's Progress in Theoretical Chemistry."

ena of dissociation are seen in a wonderful degree in the sun, the fixed stars, and the nebulæ. It is not necessary to recall to you the marvelous field of celestial chemistry which the spectroscope, in the hands of Kirchhoff and his followers, has made known to us, nor the proofs that the solar atmosphere contains in a dissociated state very many of the elements which in our own planet are met with in a free state only in the laboratory of the chemist. It is instructive to compare the spectra of the various fixed stars with each other, from white stars like Sirius, to yellow stars like Aldebaran and our own sun, and red stars like Alpha Orionis and Antares, and to note in these three classes an increasing complexity of chemical composition. In the first, with a predominance of hydrogen, we see only faint lines of magnesium, sodium, calcium, iron, and a few other metals, while in the second, though free hydrogen still abounds, the number of metallic elements is greatly augmented, and finally in the red stars hydrogen is seen only in combination, as aqueous vapor, the metals are wanting, and the metalloids and their compounds appear. If, in accordance with the nebular hypothesis, we look upon these different types of stars as representing successive stages in the process of condensation from nebula to planet, we may also see in them a gradual evolution of the more complex from the simple forms of matter by a process of celestial chemistry. Such was the view put forward by F. W. Clarke in January, 1873, and some months later by Lockyer, who has reiterated and enforced these suggestions, and, moreover, connected them with the speculations of Dumas on the composite nature of the elements. The white stars are the hottest, and in the atmosphere of these bodies the various metals, according to Lockyer, make their appearance in the order of their vapor-densities.

I ventured, in 1867, while speculating on the phenomena of dissociation, to remark that, although from the experiments of the laboratory we can only conjecture the complex nature of the so-called elementary substances, we may expect that their "further dissociation in stellar or nebulous masses may give us evidence of matter still more elemental." Now, while the nebulæ, when scanned by the spectroscope, show us only the lines of hydrogen and nitrogen, the two lightest forms of gaseous matter known to chemistry, it is remarkable that the recent studies of the solar chromosphere reveal to us the existence of an unknown gaseous element which, from its extension beyond even the layer of partially cooled hydrogen, must, according to the deductions of Mr. Johnson Stoney, be still lighter than this gas. The green line by which this substance is distinguished is not as yet identified with that of any terrestrial element. Is it not possible that we have here that more elemental form of matter which, though not seen in the nebulæ, is liberated by the intense heat of the solar sphere, and may possibly correspond to the primary matter conjectured by Dumas, having an equivalent weight one-fourth that of hydrogen? Mention

should also be made of the unknown element conjectured by Huggins to exist in some nebulæ. This conception of a first matter or *Urstoff* has also been maintained by Hinrichs, who has put forward an argument in its favor from a consideration of the wave-lengths in the lines of the spectra of various elements.

It is curious in this connection to note that Lavoisier suggested that hydrogen, nitrogen, and oxygen, with heat and light, might be regarded as simpler forms of matter from which all others were derived. The nebulæ, which we conceive as condensing into suns and planets, show us only two of the three elements of our terrestrial envelope, which is made up of air and aqueous vapor. If now we admit, as I am disposed to do with Mattieu Williams, that our atmosphere and ocean are not simply terrestrial, but cosmical, and are a portion of the medium which, in an attenuated form, fills the interstellary spaces, these same nebulæ and their resulting worlds may be evolved by a process of chemical condensation from this universal atmosphere, to which they would sustain a relation somewhat analogous to that of clouds and rain to the aqueous vapor around us. This, though it may be regarded as a legitimate and plausible speculation, is at present nothing more, and we may never advance beyond conjecture as to the relation of the various forms of so-called elemental matter, and to the processes which govern the evolution of the celestial spheres. You will, I trust, pardon this excursion to the regions of space and the realm of imagination into which I have led you, and return with me to the consideration of a new chapter in chemical theory.

REPLY TO THE CRITICS OF THE BELFAST ADDRESS.[1]

By JOHN TYNDALL, LL. D., F. R. S.

I TAKE advantage of a pause in the issue of this Address, to add a few prefatory words to those already printed.

The world has been frequently informed of late that I have raised up against myself a host of enemies; and considering, with few exceptions, the deliverances of the press, and more particularly of the religious press, I am forced sadly to admit that the statement is only too true. I derive some comfort, nevertheless, from the reflection of Diogenes, transmitted to us from Plutarch, that "he who would be saved must have good friends or violent enemies; and that he is best off who possesses both." This "best" condition, I have reason to believe, is mine.

Reflecting on the fraction I have read of recent remonstrances, ap-

[1] Preface to the seventh edition of the Address before the British Association at Belfast, with an Appendix on "Scientific Materialism," etc. D. Appleton & Co.

peals, menaces, and judgments—covering not only the world that now is, but that which is to come—it has interested me to note how trivially men seem to be influenced by what they call their religion, and how potently by that "nature" which it is the alleged province of religion to eradicate or subdue. From fair and manly argument, from the tenderest and holiest sympathy on the part of those who desire my eternal good, I pass by many gradations, through deliberate unfairness, to a spirit of bitterness which desires, with a fervor inexpressible in words, my eternal ill. Now, were religion the potent factor, we might expect a homogeneous utterance from those professing a common creed; while, if human nature be the really potent factor, we may expect utterances as heterogeneous as the characters of men. As a matter of fact we have the latter; suggesting to my mind that the common religion professed and defended by these different people is merely the accidental conduit through which they pour their own tempers, lofty or low, courteous or vulgar, mild or ferocious, holy or unholy, as the case may be. Pure abuse, however, I have deliberately avoided reading, wishing to keep, not only hatred, malice, and uncharitableness, but even every trace of irritation, far away from my side of a discussion which demands not only good temper, but largeness, clearness, and many-sidedness of mind, if it is to guide us even to provisional solutions.

At an early stage of the controversy a distinguished professor of the University of Cambridge was understood to argue—and his argument was caught up with amusing eagerness by a portion of the religious press—that my ignorance of mathematics renders me incompetent to speculate on the proximate origin of life. Had I thought his argument relevant, my reply would have been simple; for before me lies a printed document, more than twenty-two years old, bearing the signature of this same learned professor, in which he was good enough to testify that I am "well versed in pure mathematics."

In connection with his limitation of speculative capacity to the mathematician, the gentleman just referred to offered what he considered a conclusive proof of the being of a God. This solemn problem he knocked off in a single paragraph. It interests me profoundly to reflect upon the difference between the state of mind which could rest satisfied with this performance and that of the accomplished poet, and more than accomplished critic, who in "Literature and Dogma" pronounces the subject of the professor's demonstration "an unverifiable hypothesis." Whence this difference? Were the objective facts decisive, both writers would come to the same conclusion: the divergence is, therefore, to be referred to the respective subjective organs which take the outward evidence in. When I turn, as I have done from time to time for years, to the articles and correspondence in our theological journals, and try to gather from them what our religious teachers think of this universe and of each other, they seem to me to be as

far removed from nineteenth-century needs as the priests of the Homeric period. Omniscience might see in our brains the physical correlatives of our differences; and, were these organs incapable of change, the world, despite this internal commotion, would stand still as a whole. But happily that Power which, according to Mr. Arnold, "makes for righteousness" is intellectual as well as ethical; and by its operation, not as an outside but as an inside factor of the brain, even the mistaken efforts of that organ are finally overruled in the interests of truth.

It has been thought, and said, that, in the revised Address as here published, I have retracted opinions uttered at Belfast. A Roman Catholic writer, who may be taken as representative, is specially strong upon this point. Startled by the deep chorus of dissent with which my dazzling fallacies have been received, he convicts me of trying to retreat from my position. This he will by no means tolerate. "It is too late now to seek to hide from the eyes of mankind one foul blot, one ghastly deformity. Prof. Tyndall has himself told us how and where this Address of his was composed. It was written among the glaciers and the solitudes of the Swiss mountains. It was no hasty, hurried, crude production; its every sentence bore marks of thought and care."

My critic intends to be severe: he is simply just. In the "solitudes" to which he refers I worked with deliberation; endeavoring even to purify my intellect by disciplines similar to those enjoined by his own Church for the sanctification of the soul. I tried in my ponderings to realize not only the lawful, but the expedient; and to permit no fear to act upon my mind save that of uttering a single word on which I could not take my stand, either in this or any other world.

Still my time was so brief, and my process of thought and expression so slow, that, in a literary point of view, I halted, not only behind the ideal, but behind the possible. Hence, after the delivery of the Address, I went over it with the desire, not to revoke its principles, but to improve it verbally, and above all to remove any word which might give color to the notion of "heat and haste." In holding up as a warning to writers of the present the errors and follies of the denouncers of the past, I took occasion to compare the intellectual propagation of such denouncers to that of thistle-germs; the expression was thought offensive, and I omitted it. It is still omitted from the Address. There was also another passage, which ran thus: "It is vain to oppose this force with a view to its extirpation. What we should oppose, to the death if necessary, is every attempt to found upon this elemental bias of man's nature a system which should exercise despotic sway over his intellect. I do not fear any such consummation. Science has already, to some extent, leavened the world, and it will leaven it more and more. I should look upon the mild light

of science breaking in upon the minds of the youth of Ireland, and strengthening gradually to the perfect day, as a surer check to any intellectual or spiritual tyranny which might threaten this island than the laws of princes or the swords of emperors. Where is the cause of fear? We fought and won our battle even in the middle ages; why should we doubt the issue of a conflict now?"

This passage also was deemed unnecessarily warm, and I therefore omitted it. It was an act of weakness on my part to do so. For, considering the aims and acts of that renowned and remorseless organization which for the time being wields the entire power of my critic's Church, not only resistance to its further progress, but, were it not for the intelligence of Roman Catholic laymen, positive restriction of its present power for evil, might well become the necessary attitude of society as regards that organization. With some slight verbal alterations, therefore, which do not impair its strength, the passage has been restored.

My critic is very hard upon the avowal in my preface regarding atheism. But I frankly confess that his honest hardness and hostility are to me preferable to the milder but less honest treatment which the passage has received from members of other churches. He quotes the paragraph, and goes on to say: "We repeat this is a most remarkable passage. Much as we dislike seasoning polemics with strong words, we assert that this apology only tends to affix with links of steel to the name of Prof. Tyndall the dread imputation against which he struggles."

Here we have a very fair example of subjective religious vigor. But my quarrel with such exhibitions is that they do not always represent objective fact. No atheistic reasoning can, I hold, dislodge religion from the heart of man. Logic cannot deprive us of life, and religion is life to the religious. As an experience of consciousness, it is perfectly beyond the assaults of logic. But the religious life is often projected in external forms—I use the word in its widest sense—by no means beyond the reach of logic, which will have to bear—and to do so more and more as the world becomes more enlightened—comparison with facts. The subjective energy to which I have just referred is also a fact of consciousness not to be reasoned away. My critic feels, and takes delight in feeling, that I am struggling, and he obviously experiences the most exquisite pleasures of "the muscular sense" in holding me down. His feelings are as real as if his imagination of what mine are were equally real. His picture of my "struggles" is, however, a mere phantasm. I do not struggle. I do not fear the charge of atheism; nor should I even disavow it, in reference to any definition of the Supreme which he, or his order, would be likely to frame. His "links" and his "steel" and his "dread imputations" are, therefore, even more unsubstantial than my "streaks of morning cloud," and they may be permitted to vanish together.

What are the conceptions in regard to which I place myself in the position here indicated? The pope himself provides me with an answer. In the Encyclical Letter of December, 1864, his Holiness writes: "In order that God may accede more easily to our and your prayers, let us employ in all confidence, as our Mediatrix with Him, the Virgin Mary, Mother of God, who sits as a Queen on the right hand of her only-begotten Son, in a golden vestment, clothed around with various adornments."

In regard to this, as to other less pictorially anthropomorphic and sartorial conceptions of the Supreme, I stand in an attitude of unbelief; for, taken in connection with what is known of the extent, organization, and general behavior of this universe, they lack the congruity necessary to commend them to me as truth.

Soon after the delivery of the Belfast Address, the Protestant Bishop of Manchester did me the honor of noticing it; and, in reference to that notice, a brief and, I trust, not uncourteous remark was introduced into my first preface. Since that time the bishop's references to me have been very frequent. Assuredly this is to me an unexpected honor. Still a doubt may fairly be entertained whether this incessant speaking before public assemblies on emotional subjects does not tend to disturb that equilibrium of head and heart which it is always so desirable to preserve—whether, by giving an injurious predominance to the feelings, it does not tend to swathe the intellect in a warm haze, thus making the perception, and consequent rendering of facts, indefinite, if not untrue. It was to the bishop I referred in a recent brief discourse[1] as "an able and, in many respects, a courageous man, running to and fro upon the earth, and wringing his hands over the threatened loss of his ideals." It is doubtless to this sorrowing mood—this partial and, I trust, temporary overthrow of the judgment by the emotions—that I must ascribe a probably unconscious, but still grave, misrepresentation contained in the bishop's last reference to me. In the *Times* of November 9th, he is reported to have expressed himself thus: "In his lecture in Manchester, Prof. Tyndall as much as said that at Belfast he was not in his best mood, and that his despondency passed away in brighter moments." Now, considering that a *verbatim* report of the lecture was at hand in the *Manchester Examiner*, and that my own corrected edition of it was to be had for a penny, the bishop, I submit, might have afforded to repeat what I actually said, instead of what I "as much as said." I am sorry to add that his rendering of my words is a vain imagination of his own. In my lecture at Manchester there was no reference, expressed or implied, to my moods in Belfast.

To all earnest and honest minds acquainted with the paragraph of

[1] *See* THE POPULAR SCIENCE MONTHLY for January, 1875.

my first preface, on which the foregoing remark of Bishop Fraser, and similar remarks of his ecclesiastical colleagues, not to mention those of less responsible writers, are founded, I leave the decision of the question whether their mode of presenting this paragraph to the public be straightforward or the reverse.

These minor and more purely personal matters at an end, the weightier allegation remains—that at Belfast I misused my position by quitting the domain of science, and making an unjustifiable raid into the domain of theology. This I fail to see. Laying aside abuse, I hope my accusers will consent to reason with me. Is it not competent for a scientific man to speculate on the antecedents of the solar system? Did Kant, Laplace, and William Herschel, quit their legitimate spheres when they prolonged the intellectual vision beyond the boundary of experience, and propounded the nebular theory? Accepting that theory as probable, is it not permitted to a scientific man to follow up in idea the series of changes associated with the condensation of the nebulæ; to picture the successive detachment of planets and moons, and the relation of all of them to the sun? If I look upon our earth, with its orbital revolution and axial rotation, as one small issue of the process which made the solar system what it is, will any theologian deny my right to entertain and express this theoretic view? Time was when a multitude of theologians would be found to do so—when that arch-enemy of science which now vaunts its tolerance would have made a speedy end of the man who might venture to publish any opinion of the kind. But that time, unless the world is caught strangely slumbering, is forever past.

As regards inorganic Nature, then, I may traverse, without let or hinderance, the whole distance which separates the nebulæ from the worlds of to-day. But only a few years ago this now conceded ground of science was theological ground. I could by no means regard this as the final and sufficient concession of theology; and at Belfast I thought it not only my right but my duty to state that, as regards the organic world, we must enjoy the freedom which we have already won in regard to the inorganic. I could not discern the shred of a title-deed which gave any man, or any class of men, the right to open the door of one of these worlds to the scientific searcher, and to close the other against him. And I considered it frankest, wisest, and in the long-run most conducive to permanent peace, to indicate without evasion or reserve the ground that belongs to Science, and to which she will assuredly make good her claim.

Considering the freedom allowed to all manner of opinions in England, surely this was no extravagant position for me to assume. I have been reminded that an eminent predecessor of mine in the presidential chair expressed a totally different view of the Cause of things from that enunciated by me. In doing so he transgressed the bounds

of science at least as much as I did; but nobody raised an outcry against him. The freedom that he took I claim, but in a more purely scientific direction. And looking at what I must regard as the extravagances of the religious world; at the very inadequate and foolish notions concerning this universe entertained by the majority of our religious teachers; at the waste of energy on the part of good men over things unworthy, if I might say it without discourtesy, of the attention of enlightened heathens: the fight about the fripperies of Ritualism, the mysteries of the Eucharist, and the Athanasian Creed; the forcing on the public view of Pontigny Pilgrimages; the dating of historic epochs from the definition of the Immaculate Conception; the proclamation of the Divine Glories of the Sacred Heart—standing in the midst of these insanities, it did not appear to me extravagant to claim the public tolerance for an hour and a half for the statement of what I hold to be more reasonable views: views more in accordance with the verities which science has brought to light, and which many weary souls would, I thought, welcome with gratification and relief.

But to come to closer quarters. The expression to which the most violent exception has been taken is this: "Abandoning all disguise, the confession I feel bound to make before you is that I prolong the vision backward across the boundary of the experimental evidence, and discern, in that Matter which we, in our ignorance, and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the promise and potency of every form and quality of life." To call it a "chorus of dissent," as my Catholic critic does, is a mild way of describing the storm of opprobrium with which this statement has been assailed. But, the first blast of passion being past, I hope I may again ask my opponents to consent to reason. First of all, I am blamed for crossing the boundary of the experimental evidence. I reply that this is the habitual action of the scientific mind—at least of that portion of it which applies itself to physical investigation. Our theories of light, heat, magnetism, and electricty, all imply the crossing of this boundary. My paper on the "Scientific Use of the Imagination" illustrates this point in the amplest manner; and in the lecture above referred to I have sought, incidentally, to make clear how in physics the experiential incessantly leads to the ultra-experiential; how out of experience there always grows something finer than mere experience, and that in their different powers of ideal extension consists for the most part the difference between the great and the mediocre investigator. The kingdom of science, then, cometh not by observation and experiment alone, but is completed by fixing the roots of observation and experiment in a region inaccessible to both, and in dealing with which we are forced to fall back upon the picturing power of the mind.

Passing the boundary of experience, therefore, does not, in the

abstract, constitute a sufficient ground for censure. There must have been something in my particular mode of crossing it which provoked this tremendous "chorus of dissent."

Let us calmly reason the point out. I hold the nebular theory as it was held by Kant, Laplace, and William Herschel, and as it is held by the best scientific intellects of to-day. According to it, our sun and planets were once diffused through space as an impalpable haze, out of which, by condensation, came the solar system. What caused the haze to condense? Loss of heat. What rounded the sun and planets? That which rounds a tear—molecular force. For æons, the immensity of which overwhelms man's conceptions, the earth was unfit to maintain what we call life. It is now covered with visible living things. They are not formed of matter different from that of the earth around them. They are, on the contrary, bone of its bone and flesh of its flesh. How were they introduced? Was life implicated in the nebulæ —as part, it may be, of a vaster and wholly Incomprehensible Life; or is it the work of a Being standing outside the nebulæ, who fashioned it as a potter does his clay, but whose own origin and ways are equally past finding out? As far as the eye of science has hitherto ranged through Nature, no intrusion of purely creative power into any series of phenomena has ever been observed. The assumption of such a power to account for special phenomena has always proved a failure. It is opposed to the very spirit of science, and I therefore assumed the responsibility of holding up in contrast with it that method of Nature which it has been the vocation and triumph of science to disclose, and in the application of which we can alone hope for further light. Holding, then, that the nebulæ and all subsequent life stand to each other in the relation of the germ to the finished organism, I reaffirm here, not arrogantly, or defiantly, but without a shade of indistinctness, the position laid down in Belfast.

Not with the vagueness belonging to the emotions, but with the definiteness belonging to the understanding, the scientific man has to put to himself these questions regarding the introduction of life upon the earth. He will be the last to dogmatize upon the subject, for he knows best that certainty is here for the present unattainable. His refusal of the creative hypothesis *is less an assertion of knowledge than a protest against the assumption of knowledge* which must long, if not forever, lie beyond us, and the claim to which is the source of manifold confusion upon earth. With a mind open to conviction, he asks his opponents to show him an authority for the belief they so strenuously and so fiercely uphold. They can do no more than point to the Book of Genesis, or some other portion of the Bible. Profoundly interesting and indeed pathetic to me are those attempts of the opening mind of man to appease its hunger for a Cause. But the Book of Genesis has no voice in scientific questions. To the grasp of geology, which it resisted for a time, it at length yielded like potter's clay; its

authority as a system of cosmogony being discredited on all hands by the abandonment of the obvious meaning of its writer. It is a poem, not a scientific treatise. In the former aspect it is forever beautiful; in the latter aspect it has been, and it will continue to be, purely obstructive and hurtful. To *knowledge* its value has been negative, leading, in rougher ages than ours, to physical, and even in our own "free" age, as exemplified in my own case, to moral violence.

To the student of cause and effect no incident connected with the proceedings at Belfast is more instructive than the deportment of the Catholic hierarchy of Ireland; a body usually wise enough not to confer notoriety upon an adversary by imprudently denouncing him. The *Times*, to which I owe nothing on the score of sympathy, but a great deal on the score of fair play, where so much has been unfair, thinks that the Irish cardinal, archbishops, and bishops, in their recent manifesto, promptly and adroitly employed a weapon which I, at an unlucky moment, had placed in their hands. The antecedents of their action cause me to regard it in a different light; and a brief reference to these antecedents will, I think, illuminate not only their proceedings regarding Belfast, but other doings which have been recently noised abroad.

Before me lies a document, bearing the date of November, 1873, but which, after appearing for a moment, unaccountably vanished from public view. It is a memorial addressed by seventy of the students and ex-students of the Catholic University in Ireland to the Episcopal Board of the University. This is the plainest and bravest remonstrance ever addressed by Irish laymen to their spiritual pastors and masters. It expresses the profoundest dissatisfaction with the curriculum marked out for the students of the university; setting forth the extraordinary fact that the lecture-list for the faculty of Science, published a month before they wrote, did not contain the name of a single professor of the Physical or Natural Sciences.

The memorialists forcibly deprecate this, and dwell upon the necessity of education in science: "The distinguishing mark of this age is its ardor for science. The natural sciences have, within the last fifty years, become the chiefest study in the world; they are in our time pursued with an activity unparalleled in the history of mankind. Scaree a year now passes without some discovery being made in these sciences which, as with the touch of a magician's wand, shivers to atoms theories formerly deemed unassailable. It is through the physical and natural sciences that the fiercest assaults are now made on our religion. No more deadly weapon is used against our faith than the facts incontestably proved by modern researches in science."

Such statements must be the reverse of comfortable to a number of gentlemen who, trained in the philosophy of Albertus Magnus and Thomas Aquinas, have been accustomed to the unquestioning submission of all other sciences to their divine science of Theology. But

something more remains: "One thing seems certain," say the memorialists, viz., "that if chairs for the physical and natural sciences be not soon founded in the Catholic University, very many young men will have their faith exposed to dangers which the creation of a school of science in the university would defend them from. For our generation of Irish Catholics are writhing under the sense of their inferiority in science, and are determined that such inferiority shall not long continue; and so, if scientific training be unattainable at our university, they will seek it at Trinity, or at the Queen's Colleges, in not one of which is there a Catholic professor of science."

Those who imagined the Catholic University at Kensington to be due to the spontaneous recognition on the part of the Roman hierarchy of the intellectual needs of the age, will derive enlightenment from this, and still more from what follows; for the most formidable threat remains. To the picture of Catholic students seceding to Trinity and the Queen's Colleges, the memorialists add this darkest stroke of all: "They will, in the solitude of their own homes, unaided by any guiding advice, devour the works of Häckel, Darwin, Huxley, Tyndall, and Lyell; works innocuous if studied under a professor who would point out the difference between established facts and erroneous inferences, but which are calculated to sap the faith of a solitary student, deprived of a discriminating judgment to which he could refer for a solution of his difficulties."

In the light of the knowledge given by this courageous memorial, and of similar knowledge otherwise derived, the recent Catholic manifesto did not at all strike me as a chuckle over the mistake of a maladroit adversary, but rather as an evidence of profound uneasiness on the part of the cardinal, the archbishops, and the bishops who signed it. They acted toward it, however, with their accustomed practical wisdom. As one concession to the spirit which it embodied, the Catholic University at Kensington was brought forth, apparently as the effect of spontaneous inward force, and not of outward pressure which was rapidly becoming too formidable to be successfully opposed.

The memorialists point with bitterness to the fact that "the name of no Irish Catholic is known in connection with the physical and natural sciences." But this, they ought to know, is the complaint of free and cultivated minds wherever the priesthood exercises dominant power. Precisely the same complaint has been made with respect to the Catholics of Germany. The great national literature and scientific achievements of that country in modern times are almost wholly the work of Protestants; a vanishingly small fraction of it only being derived from members of the Roman Church, although the number of these in Germany is at least as great as that of the Protestants. "The question arises," says a writer in a German periodical, "what is the cause of a phenomenon so humiliating to the Catholics? It cannot be referred to want of natural endowment due to climate (for the Prot-

estants of Southern Germany have contributed powerfully to the creations of the German intellect), but purely to outward circumstances. And these are readily discovered in the pressure exercised for centuries by the Jesuitical system, which has crushed out of Catholics every tendency to free mental productiveness." It is, indeed, in Catholic countries that the weight of ultramontanism has been most severely felt. It is in such countries that the very finest spirits, who have dared, without quitting their faith, to plead for freedom or reform, have suffered extinction. The extinction, however, was more apparent than real, and Hermes, Hirscher, and Günther, though individually broken and subdued, prepared the way in Bavaria for the persecuted but unflinching Frohschammer, for Döllinger, and for the remarkable liberal movement of which Döllinger is the head and guide.

Though managed and moulded for centuries to an obedience unparalleled in any other country, except Spain, the Irish intellect is beginning to show signs of independence, demanding a diet more suited to its years than the pabulum of the middle ages. As for the recent manifesto where pope, cardinal, archbishops, and bishops, may now be considered as united in one grand anathema, its character and fate are shadowed forth by the vision of Nebuchadnezzar, recorded in the Book of Daniel. It resembles the image, whose form was terrible, but the gold, and silver, and brass, and iron of which rested upon feet of clay. And a stone smote the feet of clay, and the iron, and the brass, and the silver, and the gold, were broken in pieces together, and became like the chaff of the summer threshing-floors, and the wind carried them away.

There is something in Jesuitism profoundly interesting, and at the same time clearly intelligible, to men of strong intellects and determined will. The weaker spirits, of whom there are many among us, it simply fascinates and subdues. From the study of his own inward forces, and their possible misapplication, the really determined man can understand how possible it is, having once chosen an aim, to reach it in defiance of every moral restraint—to trample under foot, by an obstinate effort of volition, the dictates of honesty, honor, mercy, and truth; and to pursue the desired end, if need be, through their destruction. This force of will, relentlessly applied, and working through submissive instruments, is the strength of Jesuitism.

Pure, honest fanaticism often adds itself to this force, and sometimes acts as its equivalent. Illustrations of this are not far to seek, for the dazzling prize of England, converted to the true faith, is sufficient to turn weak heads. When it is safely caged, it is interesting to watch the operations of this form of energy. In a sermon on the Perpetual Office of the Council of Trent, preached before the Right Reverend Fathers assembled in Synod, the Archbishop of Westminster has given us the following sample of it: "As the fourth century was glorious by the definition of the Godhead and the Consubstantial Son,

and the fifth by that of his two perfect natures, and the thirteenth by that of the procession of the Holy Ghost, so the nineteenth will be glorious by the definition of the Immaculate Conception. Right Rev. Fathers," continues this heated proselyte, "you have to call the legionaries and the tribunes, the patricians and the people, of a conquering race, and to subdue, change, and transform them one by one to the likeness of the Son of God. Surely a soldier's eye and a soldier's heart would choose by intuition this field of England for the warfare of the faith. It is the head of Protestantism, the centre of its movements, and the stronghold of its powers. Weakened in England, it is paralyzed everywhere; conquered in England, it is conquered throughout the world. Once overthrown here, all is but a war of detail: it is the key of the whole position of modern error." This is the propaganda which England has to stem. What mere stubble a *dilettante* ritualist or a weak-headed nobleman must be when acted upon by this fiery breath of fanaticism! The only wonder is that weak heads, which are so assiduously and deliberately sought out, are not more plentiful than they are.

Monsignor Capel has recently been good enough to proclaim at once the friendliness of his Church toward true science, and her right to determine what true science is. Let us dwell for a moment on the historic proofs of her scientific competence. When Halley's comet appeared in 1456, it was regarded as the harbinger of God's vengeance, the dispenser of war, pestilence, and famine, and, by order of the pope, all the church-bells in Europe were rung to scare the monster away. An additional daily prayer was added to the supplications of the faithful. The comet in due time disappeared, and the faithful were comforted by the assurance that, as in previous instances relating to eclipses, droughts, and rains, so also as regards this "nefarious" comet, victory had been vouchsafed to the Church.

Both Pythagoras and Copernicus had taught the heliocentric doctrine—that the earth revolved round the sun. In the exercise of her right to determine what true science is, the Church, in the pontificate of Paul V., stepped in, and, by the mouth of the holy Congregation of the Index, delivered, on March 5, 1616, the following decree:

And whereas it hath also come to the knowledge of the said holy congregation that the false Pythagorean doctrine of the mobility of the earth and the immobility of the sum, entirely opposed to Holy Writ, which is taught by Nicolas Copernicus, is now published abroad and received by many—in order that this opinion may not further spread, to the damage of Catholic truth, it is ordered that this and all other books teaching the like doctrine be suspended, and by this decree they are all respectively suspended, forbidden, and condemned.

Though often quoted, I thought the never-dying flavor of this celebrated decree would not be disagreeable to some of my readers. It is pleasant to be able to say that the very doctrine here pronounced

"false," "opposed to Holy Writ," and "damaging to Catholic truth," Science has persuaded even Monsignor Capel to accept.

But it is a constant *tendency* rather than a single fact which is chiefly important here, and a few jottings will show with sufficient plainness what this tendency has ever been. The fate of Giordano Bruno is referred to in my Belfast Address. For a further reference to him I would direct the reader to a brief passage in the Appendix to the same. The case of Galileo is also touched upon; and to this it may be added here that he died the prisoner of the Inquisition, which, true to its instincts, followed him beyond the grave, disputing his right to make a will, and denying him burial in consecrated ground.[1]

Again, the famous Academia del Cimento was established at Florence in 1657, and held its meetings in the ducal palace. It lasted ten years, and was then suppressed at the instance of the Papal Government. As an equivalent, the brother of the grand-duke was made a cardinal. The Jesuits were less successful in Bavaria in 1759; for they did their best, but vainly, to prevent the founding of the Academy of Sciences in Munich. Their waning power was indicated by this fact, and in 1773 Pope Clement XIV. dissolved the order. The decree was to be "irrevocable;" the Society of Jesus was "never to be restored;" still, in 1814, an infallible follower of Clement, Pope Pius VII., undid the work of his equally infallible predecessor, and revoked his decree.

But why go back to 1456? Far be it from me to charge by-gone sins upon Monsignor Capel's Church, were it not for her practices to-day. The most applauded dogmatist of the Jesuits is, I am informed, Perrone. Thirty editions of a work of his have been scattered abroad in all lands by a society to which he belongs. His notions of physical astronomy are quite in accordance with those of 1456. He teaches boldly that "God does not rule by universal law . . . . that when God [obviously a Big Man] orders a given planet to stand still he does not detract from any law passed by himself, but orders that planet to move round the sun for such and such a time, then to stand still, and then again to move, as his pleasure may be." Jesuitism proscribed Frohschammer for questioning its favorite dogma that every human soul was created by a direct supernatural act of God, and for asserting that man, body and soul, came from his parents. This is the society that now strives for universal power; it is from it, as Monsignor Capel graciously informs us, that we are to learn what is allowable in science and what is not!

In the face of such facts, which might be multiplied at will, it requires extraordinary bravery of mind, or a reliance upon public ignorance almost as extraordinary, to make the claims made by Monsignor Capel for his Church.

A German author, speaking of one who has had bitter experience

[1] Draper, "Trial of Galileo."

in this line, describes those Catholic writers who refuse to submit to the Congregation of the Index as outlawed; fair subjects for moral assassination.[1] This is very strong; and still, judging from my own small experience, not too strong. In reference to this point I would ask indulgence for a brief personal allusion here. It will serve a two-fold object, one of which will be manifest, the other being reserved for possible future reference. Sprung from a source to which the Bible was specially dear, my early training was confined almost exclusively to it. Born in Ireland, I, like my predecessors for many generations, was taught to hold my own against the Church of Rome. I had a father whose memory ought to be to me a stay, and an example of unbending rectitude and purity of life. The small stock to which he belonged were scattered with various fortunes along that eastern rim of Leinster, from Wexford upward, to which they crossed from the Bristol Channel. My father was the poorest of them. Still, in his socially low but mentally and morally independent position, by his own inner energies and affinities, he obtained a knowledge of history which would put mine to shame; while the whole of the controversy between Protestantism and Romanism was at his finger's ends. At the present moment the works and characters which occupied him come, as far-off recollections, to my mind: Claude and Bossuet, Chillingworth and Nott, Tillotson, Jeremy Taylor, Challoner and Milner, Pope and McGuire, and others whom I have forgotten, or whom it is needless to name. Still this man, so charged with the ammunition of controversy, was so respected by his Catholic fellow-townsmen, that they one and all put up their shutters when he died.

With such a preceptor, and with an hereditary interest in the papal controversy, I naturally mastered it. I did not confine myself to the Protestant statement of the question, but made myself also acquainted with the arguments of the Church of Rome. I remember to this hour the interest and surprise with which I read Challoner's "Catholic Christian Instructed," and on the border-line between boyhood and manhood I was to be found taking part in controversies in which the rival faiths were pitted against each other. I sometimes took the Catholic side, and gave my Protestant antagonist considerable trouble. The views of Irish Catholics became thus intimately known to me, and there was no doctrine of Protestantism which they more emphatically rejected, and the ascription of which to them they resented more warmly, than the doctrine of the pope's personal infallibility. Yet, in the face of this knowledge, it was obstinately asserted and reasserted in my presence some time ago, by a Catholic priest, that the doc-

[1] *See* the case of Frohschammer as sketched by a friend in the Preface to "Christenthum und die moderne Wissenschaft." His enemies contrived to take his bread, in great part, away, but they failed to subdue him, and not even the Pope's nuncio could prevent five hundred students of the University of Munich from signing an address to their professor.

trine of the infallibility of the pope had always been maintained in Ireland.[1]

But this is an episode, intended to disabuse those who, in this country or the United States, may have been misled in regard to the personal points referred to. I now return to the impersonal. The course of life upon earth, as far as Science can see, has been one of amelioration—a steady advance on the whole from the lower to the higher. The continued effort of animated Nature is to improve its conditions and raise itself to a loftier level. In man, improvement and amelioration depend largely upon the growth of conscious knowledge, by which the errors of ignorance are continually moulted and truth is organized. It is assuredly the advance of knowledge that has given a materialistic color to the philosophy of this age. Materialism is, therefore, not a thing to be mourned over, but to be honestly considered—accepted if it be wholly true, rejected if it be wholly false, wisely sifted and turned to account if it embrace a mixture of truth and error. Of late years the study of the nervous system and of its relation to thought and feeling has profoundly occupied inquiring minds. It is our duty not to shirk—it ought rather to be our privilege to accept—the established results of such inquiries, for here assuredly our ultimate weal depends upon our loyalty to the truth. Instructed as to the control which the nervous system exercises over man's moral and intellectual nature, we shall be better prepared, not only to mend their manifold defects, but also to strengthen and purify both. Is mind degraded by this recognition of its dependence? Assuredly not. Matter, on the contrary, is raised to the level it ought to occupy, and from which timid ignorance would remove it.

But the light is dawning, and it will become stronger as time goes on. Even the Brighton Congress affords evidence of this. From the manifold confusions of that assemblage my memory has rescued two items which it would fain preserve: the recognition of a relation between Health and Religion, and the address of the Rev. Harry Jones. Out of the conflict of vanities his words emerge fresh, healthy, and strong, because undrugged by dogma, coming directly from the warm brain of one who knows what practical truth means, and who has faith in its vitality and inherent power of propagation. I wonder is he less effectual in his ministry than his more embroidered colleagues? It surely behooves our teachers to come to some definite understanding as to this question of health: to see how, by inattention to it, we are defrauded, negatively, by the privation of that "sweetness and light" which is the natural concomitant of good health; positively, by the insertion into life of cynicism, ill-temper, and a thousand corroding

[1] On a memory which dates back to my fifteenth year, when I first read the discussion between Mr. Pope and Father McGuire, I should be inclined to rely for proof that the Catholic clergyman, in that discussion, and in the name of his Church, repudiated the doctrine of personal infallibility.

anxieties which good health would dissipate. We fear and scorn "materialism." But he who knew all about it, and could apply his knowledge, might become the preacher of a new gospel. Not, however, through the ecstatic moments of the individual does such knowledge come, but through the revelations of science, in connection with the history of mankind.

Why should the Roman Catholic Church call gluttony a mortal sin? Why should prayer and fasting occupy a place in the disciplines of a religion? What is the meaning of Luther's advice to the young clergyman who came to him, perplexed with the difficulty of predestination and election, if it be not that, in virtue of its action upon the brain, when wisely applied, there is moral and religious virtue even in a hydro-carbon? To use the old language, food and drink are creatures of God, and have therefore a spiritual value. The air of the Alps would be augmented tenfold in purifying power if this truth were recognized. Through our neglect of the monitions of a reasonable materialism we sin and suffer daily. I might here point to the train of deadly disorders over which science has given modern society such control—disclosing the lair of the material enemy, insuring his destruction, and thus preventing that moral squalor and hopelessness which habitually tread on the heels of epidemics in the case of the poor.

Rising to higher spheres, the visions of Swedenborg, and the ecstasy of Plotinus and Porphyry, are phases of that psychical condition, obviously connected with the nervous system and state of health, on which is based the Vedic doctrine of the absorption of the individual into the universal soul. Plotinus taught the devout how to pass into a condition of ecstasy. Porphyry complains of having been only once united to God in eighty-six years, while his master Plotinus had been so united six times in sixty years.[1] A friend who knew Wordsworth informs me that the poet, in some of his moods, was accustomed to seize hold of an external object to assure himself of his own bodily existence. The "entranced mind" of Mr. Page-Roberts, referred to so admiringly by the *Spectator*, is a similiar phenomenon. No one, I should say, has had a wider experience in this field than Mr. Emerson. As states of consciousness those phenomena have an undisputed reality, and a substantial identity. They are, however, connected with the most heterogeneous objective conceptions. Porphyry wrote against Christianity; Mr. Page-Roberts is a devout Christian. But notwithstanding the utter discordance of these objective conceptions, their subjective experiences are similar, because of the similarity of their finely-strung nervous organizations.

But, admitting the practical facts, and acting on them, there will always remain ample room for speculation. Take the argument of the Lucretian. As far as I am aware, not one of my assailants has attempted to answer it. Some of them, indeed, rejoice over the ability

[1] *See* Dr. Draper's important work, "Conflict between Religion and Science."

displayed by Bishop Butler in rolling back a difficulty on his opponent; and they even imagine that it is the bishop's own argument that is there employed. Instructed by self-knowledge, they can hardly credit me with the wish to state both sides of the question at issue, and to show, by a logic stronger than Butler ever used, the overthrow which awaits any doctrine of materialism which is based upon the definitions of matter habitually received. But the raising of a new difficulty does not abolish—does not even lessen—the old one, and the argument of the Lucretian remains untouched by any thing the bishop has said or can say.

And here it may be permitted me to add a word to an important controversy now going on. In an article on "Physics and Metaphysics," published in the *Saturday Review* more than fourteen years ago, I ventured to state thus the relation between physics and consciousness: "The philosophy of the future will assuredly take more account than that of the past of the relation of thought and feeling to physical processes; and it may be that the qualities of Mind will be studied through the organism as we now study the character of Force through the affections of ordinary matter. We believe that every thought and every feeling has its definite mechanical correlative in the nervous system—that it is accompanied by a certain separation and remarshaling of the atoms of the brain.

"This latter process is purely physical; and were the faculties we now possess sufficiently strengthened, without the creation of any new faculty, it would doubtless be within the range of our augmented powers to infer from the molecular state of the brain the character of the thought acting upon it, and, conversely, to infer from the thought the exact corresponding molecular condition of the brain. We do not say—and this, as will be seen, is all-important—that the inference here referred to would be an *a priori* one. What we say is, that by observing, with the faculties we assume, the state of the brain, and the associated mental affections, both might be so tabulated side by side, that if one were given, a mere reference to the table would declare the other.

"Given the masses of the planets and their distances asunder, and we can infer the perturbations consequent on their mutual attractions. Given the nature of a disturbance in water, air, or ether, and from the physical properties of the medium we can infer how its particles will be affected. The mind runs along the line of thought which connects the phenomena, and, from beginning to end, finds no break in the chain. But, when we endeavor to pass by a similar process from the phenomena of physics to those of thought, we meet a problem which transcends any conceivable expansion of the powers we now possess. We may think over the subject again and again—it eludes all intellectual presentation—we stand, at length, face to face with the Incomprehensible."

The discussion above referred to turns on the question: Do states

of consciousness enter as links in the chain of antecedence and sequence which give rise to bodily actions and to other states of consciousness; or are they merely by-products, which are not essential to the physical processes going on in the brain? Now, it is perfectly certain that we have no power of imagining states of consciousness interposed between the molecules of the brain, and influencing the transference of motion among the molecules. The thought "eludes all mental presentation;" and hence the logic seems of iron strength which claims for the brain an automatic action, uninfluenced by states of consciousness. But it is, I believe, admitted by those who hold the automaton-theory that states of consciousness are *produced* by the marshaling of the molecules of the brain; and this production of consciousness by molecular motion is certainly quite as unthinkable as the production of molecular motion by consciousness. If, therefore, unthinkability be the proper test, we must equally reject both classes of phenomena. I, for my part, reject neither, and thus stand in the presence of two Incomprehensibles, instead of one Incomprehensible. While accepting fearlessly the facts of materialism dwelt upon in these pages, I bow my head in the dust before that mystery of the brain which has hitherto defied its own penetrative power, and which may ultimately resolve itself into a demonstrable impossibility of self-penetration.[1]

But, whatever be the fate of theory, the practical monitions are plain enough, which declare that on our dealings with matter depends our weal or woe, physical and moral. The state of mind which rebels against the recognition of the claims of "materialism" is not unknown to me. I can remember a time when I regarded my body as a weed, so much more highly did I prize the conscious strength and pleasure derived from moral and religious feeling, which, I may add, was mine without the intervention of dogma. The error was not an ignoble one, but this did not save it from the penalty attached to error. Saner knowledge taught me that the body is no weed, and that if it were treated as such it would infallibly avenge itself. Am I personally lowered by this change of front? Not so. Give me their health, and there is no spiritual experience of those earlier years—no resolve of duty, or work of mercy, no act of self-denial, no solemnity of thought, no joy in the life and aspects of Nature, that would not still be mine. And this without the least reference or regard to any purely personal reward or punishment looming in the future.

As I close these remarks, the latest melancholy wail of the Bishop of Peterborough reaches my ears. Notwithstanding all their "expansiveness," both he and his brother of Manchester appear, alas! to know as little of the things which belong to our peace as that wild ritualist who, a day or two ago, raised the cry of "excommunicated heretic!" against the Bishop of Natal. Happily we have among us

[1] *See* Tyndall's "Fragments of Science," article "Scientific Materialism."

our Jowetts and our Stanleys, not to mention other brave men, who see more clearly the character and magnitude of the coming struggle; and who believe undoubtingly that out of it the truths of science will emerge with healing in their wings. Such men must increase, if the vast material resources of the Church of England are not to fall into the hands of persons who may be classed under the respective heads of *weak* and *infatuated*.

And now I have to utter a "farewell," free from bitterness, to all my readers—thanking my friends for a sympathy more steadfast, I would fain believe, if less noisy, than the antipathy of my foes; commending to these, moreover, a passage from Bishop Butler, which they have either not read or failed to take to heart. "It seems," saith the bishop, "that men would be strangely headstrong and self-willed, and disposed to exert themselves with an impetuosity which would render society insupportable, and the living in it impracticable, were it not for some acquired moderation and self-government, some aptitude and readiness in restraining themselves, and concealing their sense of things." In this respect, at least, his grace the Archbishop of Canterbury has set a good example.

WATER-SUPPLY OF ISLANDS.

By FRANKLIN C. HILL.

ON islands of considerable size and height, composed of rocks and various earthy beds, springs of fresh water in the valleys are not uncommon, and their presence excites no remark. The rainfall of the island itself is laid up in its strata exactly as in the hills of the main-land, and the small size of the reservoir is made up for by the frequent rains and fogs to which islands are subject.

There are cases of islands near the main-land where springs are fed by streams from the continent following the rock-strata below the dividing straits. But on islands composed as many of those on our Southern coasts are, of pure sand and of very small elevation, and hence with no raised reservoir to supply springs, the fact that pure fresh water may be obtained in large quantities by digging, is a mystery even to many well-informed people, although the explanation is very simple. To say that the "sea-water filters through the sand into these wells and becomes as sweet and pure as spring-water," is simply to display profound ignorance of chemistry and facts.

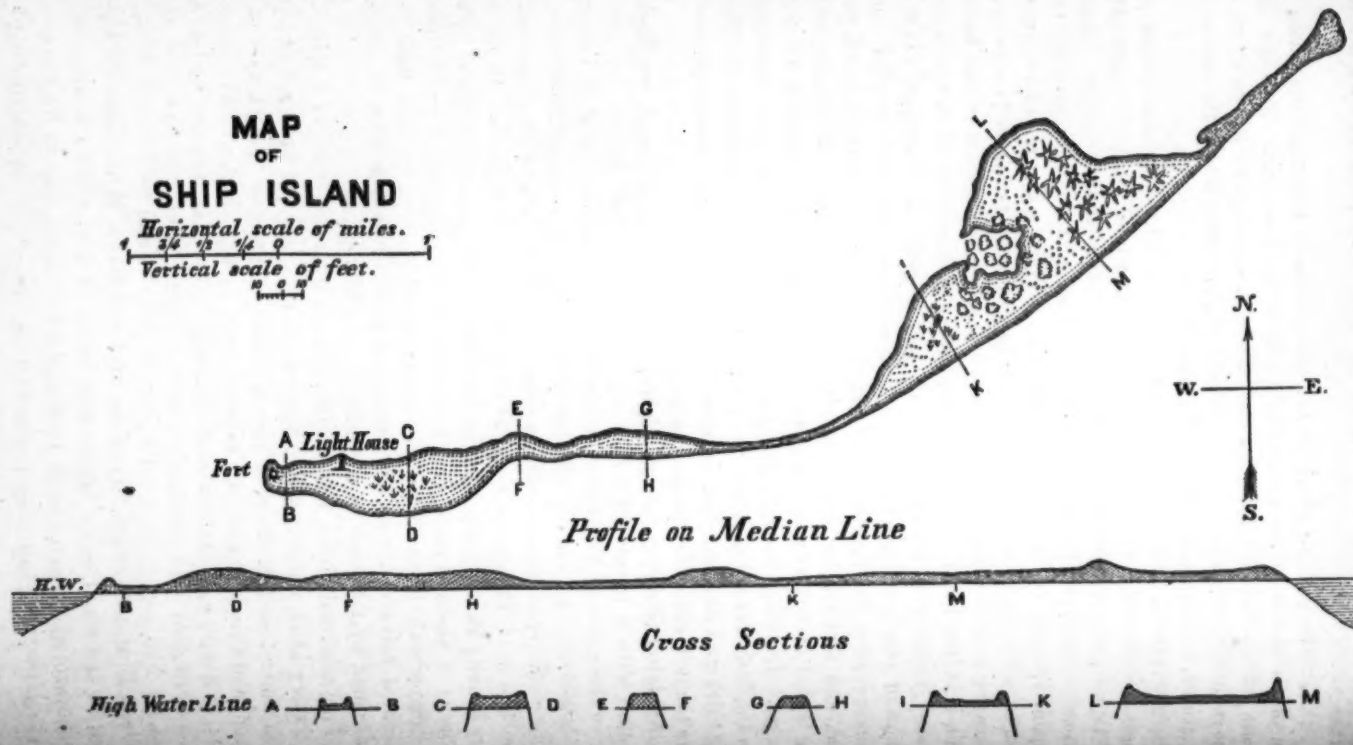
From time immemorial the ash-leach has been in use in many civilized, that is, soap-making, countries. Essentially an ash-leach is a vessel tight enough to hold wood-ashes, but not tight enough to hold water. Being first filled with ashes, water is then poured in gradually,

and, after a time, runs out below, highly charged with the soluble salt of the ashes.

But, although this machine has been so long in use, the principle on which it acts does not seem to have been fully understood until quite lately. About the year 1833 Messrs. Boullay, of Paris, applied the same apparatus to the manufacture of pharmaceutical preparations, tinctures, infusions, extracts, etc., and it at once became popular with pharmaceutists, under the name of Boullays' Filter, or the Displacement Apparatus. Its mode of operation is simple: the first portion of liquid poured in sinks into the powder that is to be exhausted, and saturates itself with the soluble parts of it. The later additions of liquid, instead of mixing with the first, drive it down before them and take its place, to yield it in turn to the next portions poured in. Thus the first portions of liquid that run from the bottom of the filter will, if it has been properly managed, contain nearly all the soluble matter, and the last will be almost unchanged. For example, if an ounce of powdered ginger be put into a glass tube, as a small lamp-chimney, over the lower end of which a piece of cotton cloth has been tied, and alcohol be slowly poured through it, the first fluid-ounce (f℥j) that comes through will contain about all the strength of the ginger. Looking through the glass, we can watch the whole process, see the first alcohol dissolve the resinous matter of the ginger, becoming thick and dark-colored in consequence, and then falling down before the new colorless alcohol added above.

Applying this principle of displacement to a sand-island, we have to start with a great heap of sand with its level or hollow top just above the reach of the waves, while the great mass of sand is below the sea-level. Of course all the submerged part is full of salt-water, while capillary attraction carries some of the water above the sea-level. The first rain that falls sinks at once into the loose sand. It rains very heavily in the gulf islands, but I never saw water run off of the surface; it sinks at once, and, in sinking, drives the salt-water before it, "displaces" it as the apothecaries say, but does not mix with it. Repeated rains continue the operation until there is a lake of fresh water held by the sand in the midst of the sea.

Fine sand will hold, between its grains, over one-third of its bulk of water; thus, within twelve feet of the sea-level, we have a lake of fresh water four feet deep, or about 1,250,000 gallons to each acre of island, and this is the supply reached by digging. But, should a long drought occur, evaporation would rapidly reduce the supply, and the sea-water following in will take its old place. Hence it was that, in 1864, we had plenty of good water at Ship Island and Santa Rosa, while at Brazos Santiago, where no rain had fallen, at the time of my visit, for ten months, the water was brackish and unfit for use, and the government was obliged to distill water from the sea for the use of the army, and the only vegetation on the island appeared to be


MAP
OF
SHIP ISLAND
Horizontal scale of miles.
Vertical scale of feet.
A Light House
Fort
B
C
D
E
F
G
H
I
K
L
M
N.
W.
E.
S.
Profile on Median Line
H.W.
Cross Sections
High Water Line

cactus and mesquite, showing that drought was not of rare occurrence there.

Our men encamped on Ship Island complained that the water in many of the wells which they dug soon became bad. Perhaps this was owing to the surface-water of the swamps getting into them, or perhaps the drainage of the camps. Those who know the habits of our men in camp can judge for themselves.

A glance at the map and profiles of Ship Island will show how admirably the island is adapted to collecting and holding rain-water. The broader parts of the island are completely surrounded by a raised beach, making large basins. The basin west of the lighthouse is comparatively new. All the islands in that chain grow at their western ends and wash away on the east. The only vegetation in this western basin was, at the time of my sojourn there, a few low, creeping herbs. The beach was so low that a heavy September gale blew the waves over it, and the whole basin became a salt lake, around which I walked the next day on the beach. The sea having by that time fallen rather below its usual height, there was a difference of several feet between it and the surface of the lake. Observing that at one place the water was just level with the top of the beach, I scratched a shallow channel across it with a stick. In five minutes a strong brook was running out, and, in ten more, a roaring river. This part of the island being subject to such overflows, of course no wells are sunk in it. The fort depends on cisterns of rain-water; but it would be easy to run a pipe underground to a well above the lighthouse and get plenty of water.

The basin east of the lighthouse was a swamp in my time, thickly overgrown with grasses. Afterward, the commandant had it ploughed and made into a garden, said to have been very productive, especially in melons. The neck, of course, was barren, shifting sand, while the large basin beyond was not only swampy, but had a fresh pond in it. The drier parts were covered with a beautiful purple-topped grass, and had some showy flowers; while the wetter parts, and even the pond, were thickly set with woody shrubs of considerable size, and the wide, shallow mouth of the pond was so obstructed by them that the quiet waters of the Mississippi Sound never penetrated it. Beyond the pond were formerly some live-oaks and pines; but the oaks had all been cut down, and the pines were rapidly following them, being carried off for fuel by the prison-camp near the light-house. All around this end of the island, stumps and dead trees standing far out in the water showed that the sand was being gradually swept away by the waves.

Santa Rosa is Ship Island on a larger scale. It has several fresh-water ponds, and many bushes and trees. On its barren western end I obtained an abundance of good water by sinking a well some four or five feet. The wooden curb was built larger at the bottom, and set in

a shallow pit scooped in the sand. A man was then put inside to shovel out the sand, and, as he dug, the curb sank around him. Presently he was waist-deep in water, and the well was finished and yielded freely all summer.

MAREY'S NEW RESULTS IN ANIMAL MOVEMENTS.

By Prof. ALFRED M. MAYER,
OF THE STEVENS INSTITUTE OF TECHNOLOGY.

THE publication of Marey's "Animal Mechanism" in the "International Scientific Series" has put the general reader in possession of one of the most interesting works ever published on experimental physiology. The simplicity and precision of the author's experimental methods, his conscientiousness in being sure of one step before he takes the next, and the skill displayed in interpreting and combining his experimental results—all these admirable characteristics have rendered his book instructive and entertaining to those who merely follow from afar the progress of science, while, at the same time, he has furnished a model of precise research and clear exposition to the professed scientist.

Marey arrives at his facts *directly*, not *inferentially*, and this is the charm of his book. The mind of the reader does not rest on the fallible judgment or mere opinion of the author, but is brought face to face with the very records made by the phenomena themselves.

In studying the progress of science, one cannot help remarking certain periods of sudden acceleration in the progress of discovery. These periods of unusual activity are not always, but certainly are very often, due to the invention of some precise and readily-applicable instrument, which gives, as it were, a new scientific sense, and brings into the range of our intellectual vision phenomena and numbers whose existence were barely suspected, until revealed by the aid of some comparatively simple contrivance. Such epochs of sudden progress followed the inventions of the telescope, the spectroscope, the ophthalmoscope, the galvanometer, and the tuning-fork chronoscope. For the latter instrument, men of science are indebted to Dr. Thomas Young, that wonderful man, who touched no department of knowledge that he did not adorn. The application of the sinuous traces of a vibrating tuning-fork on a rolling cylinder, to divide a second of time into as many parts as the number of times the fork swings to and fro in a second, was described by Young in 1807, and published in his "Lectures on Natural Philosophy and the Mechanical Arts," vol. i., p. 191. Like Young's discoveries of the theory of colors, and of the undulatory theory of light, this beautiful invention laid fallow for many years, until reinvented in 1840 by Duhamel, and subsequently brought into general use in physics and physiology. It is now the

essential element of the chronoscopes, or, more accurately speaking, of the *chronometers*, used to measure the velocities of projectiles, and to solve such problems as the rates of progress of the nervous influence and of the muscular wave.

To make any tuning-fork a chronoscope, it is only necessary to know the number of vibrations which the fork makes in a second at a known temperature. This number is determined to the last degree of precision by the following method, devised by the author of this article: A break-circuit clock is placed in the primary or battery circuit of an induction-coil; while one terminal wire of the secondary coil is connected with a metallic cylinder covered with smoked paper, the other terminal wire is led to the tuning-fork, which traces its vibrations, by means of a delicate metallic point, on the paper-covered cylinder. At each second the break-circuit clock sends a spark from the point attached to the vibrating fork, through the smoked paper to the metallic cylinder. It is evident that, on counting the number of sinuosities made by the vibrating fork between two contiguous spark-holes, we have the number of vibrations per second made by the fork. After the above determination has been made, the tuning-fork becomes the most accurate and uniformly rated chronometer yet devised by men of science. But the time-recording tuning-fork is only one part of the apparatus required in the study of physiological motions. We must also be in possession of some contrivance which can be readily applied to an organ, the durations and varied velocities of whose motions we would study, and this contrivance must make a graphic record of these motions alongside of the time-record drawn by the tuning-fork. To Marey we are indebted for many effective recording instruments, but the apparatus which he has most extensively used, and which is admirably adapted to the study of the motions in some of the vital functions and in locomotion, consists of a small drum of shallow depth, one of whose ends is covered with an elastic membrane. The interior of this drum is connected with the interior of a similar drum by a rubber tube of very small internal diameter. The membrane of one of these drums presses against the surface whose motions we would study. A delicate lever rests on the membrane of the other drum, and the end of this lever is armed with a delicate point which touches a revolving cylinder covered with smoked paper. On this cylinder the tuning-fork also simultaneously traces its time-record. Now, as both drums, and the tube which connects them, form an air-tight space, it follows that any depression, given to the membrane which touches the moving surface, will compress the air in this drum, in the connecting tube, and in the drum which carries the delicate lever. The membrane of the latter drum will move outward and cause the pointed lever to move and make its trace on the revolving cylinder. Of course an elevation of the membrane in the first drum will cause a depression in the membrane of the second drum, accompanied by a

movement of the lever opposite to that described above. Thus the lever records accurately every movement of the membrane of the distant drum, and the intervening flexible tube allows one to attach the drum to the limb of a moving man, or quadruped, to the wing of a flying-bird, or to the chest, to obtain the traces of the motions of the lungs and of the heart.

In 1863 Marey first began to apply the graphic method to biological studies, in his "Physiologie médicale de la Circulation du Sang." In 1868 he published his "Du Mouvement dans les Fonctions de la Vie." In the preface of this truly valuable work he says: "By the use of the graphic method the illusions of the observer, the tediousness of descriptions, and the confusion of facts, disappear. These two ruling qualities, clearness and conciseness, become every day more desirable, by reason of the enormous increase in biological publications." In his last work, "Animal Mechanism," he has illustrated this remark; for surely no "tediousness" will be experienced in the perusal of this work, in which we are taught, with such "clearness and conciseness," how men and quadrupeds walk and run, and how birds and insects fly.

The desire to see Marey's work on Animal Mechanism fully appreciated by the public has induced us to put the reader in possession of his quite recent discoveries, which could not be incorporated in the book published in the "International Series." We refer to two of his most important researches, one on "Human Locomotion," taken from the *Comptes Rendus*, of July 13, 1874; the other, "On the Resistance of the Air under the Wing of a Bird during its Flight," we take from the *Journal de Physique* of July, 1874.

I. New Experiments on Human Locomotion.—The brothers Weber believed that in human locomotion the oscillation of the leg in walking was due alone to the action of gravity; this is to say, that the foot, while off the ground, has the motion of a pendulum. For a long time this opinion has held its place in physiology, but it has been opposed, in recent years, by arguments of various kinds. First, by M. Duchenne, of Boulogne, who showed that the leg is not entirely passive during its displacement, for certain muscular paralyses prevent its oscillation; M. Giraud-Teulon has attacked the theory of Weber, by showing the mathematical errors on which it is founded; and, finally, M. Carlet has determined, experimentally, the active function of certain muscles in the displacement of the leg during walking.

If gravity does not alone act in producing the oscillation of the leg, it becomes impossible to foresee what motion will result from its combination with muscular action. I have appealed to the graphic method for the experimental answer to this question.

When a body moves in a straight line, with variable velocities at each instant, it is easy to obtain the graphic representation of its motion, provided the space moved over is not too extensive. It

suffices to join the body, by means of a rigid connection, with a writing-lever, which touches a revolving cylinder, covered with smoked paper. The tracer on the writing-lever, moved with variable velocities, and in a direction parallel to the axis of the cylinder, will draw sinuous curves, whose parts will indicate by their inclination the velocities of the motions which produced them.

But the motions in walking are too extended to be traced on the revolving cylinder in their real magnitudes; in order to reduce them, while at the same time I preserved their characteristics unaltered, I had recourse to a train of wheel-work. In this apparatus, each wheel working into another, whose teeth are ten times more numerous than those of the former, it follows that the motion communicated to the first axis will be reproduced by the second with a reduction of $\frac{1}{10}$; the third axis will reproduce the motion reduced to $\frac{1}{100}$; and the fourth axis will reduce it to $\frac{1}{1000}$, etc.

If we attach to the foot of a walker a thread, which is wrapped around the wheel on the first axle of the wheel-work, and if to the third axle we connect the writing-lever, we can obtain traces on the revolving cylinder which will have only $\frac{1}{100}$th of the extent of the paths gone over by the foot of the walker.

Fig. 1 shows five traces obtained from the foot when walking with various velocities. *A* has been produced by the slowest walk; *B* is the ordinary gait; while *C* is the most rapid: the remaining traces have been obtained from gaits less rapid than that of *C*.

FIG. 1.

(The figure represents the smoked paper, unrolled from the revolving cylinder after the experiment. The paper revolved with the cylinder in the direction from *O* to *Y*. Therefore, the axis of the cylinder was parallel to *OX*, and the tracer on the writing-lever moved parallel to *OX*. It follows that, if the foot had remained stationary while the cylinder revolved, the tracer would have described a straight line parallel to *OY*. *CH* is the trace of the vibrating tuning-fork; each bend of its sinuous line is equal to $\frac{1}{10}$th of a second of time. This chronographic trace gives us the means of estimating accurately the

duration of each step and the velocity of the foot at each instant while it is swinging in the air. The spaces gone over by the tracer, as before stated, are $\frac{1}{100}$th of the real distances traversed by the foot; that is, one centimetre on the paper equals one metre gone over by the foot.) Hence, every thing relative to the transport of the foot in walking is expressed in this figure.

1. *Velocity of the Gait.*—This is expressed by the general inclination of the curve, or by the relation existing between the lengths parallel to *OX* and to *OY*. As the different traces contained in the figure correspond to the same distance (three metres and a half; marked on the left-hand vertical line of the figure) gone over in variable times, it follows that the relation of these times to this distance will give the velocities of the different gaits. If we count on the chronograph-trace the time included between the beginning of each curve, and its termination in the line *A*, *B*, *C*, we shall have the measure of this time. (For example, the time occupied in going over 3½ metres with the gait *B* is given by counting the bends of the tuning-fork trace contained between 2 and the perpendicular line let fall from *B* on to the chronograph-trace.) Thus, for the slow walk from 1 to *A*, we count thirteen seconds; the more rapid walk from 2 to *B* occupied six and a half seconds; while with the rapid gait the distance from 5 to *C* was traversed in two seconds.

2. *Alternate Periods of Rest and of Motion of the Foot.*—It is evident that, whenever the traces show an horizontal line (that is, a line parallel to *OY*), those portions of the traces correspond to the traces made while the foot touched the ground and was immovable, since the spaces then gone over are nothing. The traces show that the duration of the periods of repose decreases as the gait is accelerated. The time during which the foot is in motion is shown by the oblique lines whose projection on the trace of the chronograph increases, relatively to the periods of repose, as the gait is more rapid. This proves that the length of the step increases with the velocity of the gait.

We can also, from the traces, estimate with precision the relation of the velocity of gait to the length of step, the relative variations of the duration of the periods of repose and of motion of the foot, etc.; but we will not here dwell on these details; the essential point under consideration is the following:

3. *The Nature of the Movement of Translation of the Foot.*—The trace of this movement is shown in a line which is nearly straight in all of its parts; the motion of the foot is therefore uniform during nearly the whole of its translation; the inflections of the line at its beginning and at its end show that, in rapid gaits especially, the motion of the foot begins and ends in short periods of variable velocities. From the above we are now able to judge how far the oscillation of the leg is analogous to that of a pendulum.

But we must not exclusively attribute to the action of the muscles of the leg this uniformity in the translation of the foot. In fact, we know that, during this translation, two distinct causes are working:

1. The angular movement which the leg has around the pelvis.

2. The horizontal translation of the pelvis itself; that is to say, of the point of suspension of the leg while the latter oscillates.

We may conceive that, by the combination of these two movements, the motion of the leg may tend to become uniform; this will happen if the minima of the velocities due to the first-named species of motion correspond with the maxima of the second kind of motion. It therefore becomes very interesting to determine what is really the motion of the trunk of the body during different gaits.

The apparatus already described has also served for the solution of this problem.

A cord attached to the waist transmitted to the registering apparatus the motion of translation of the trunk. By experimenting successively on various gaits, we obtained the following figure, whose analysis gives some interesting results:

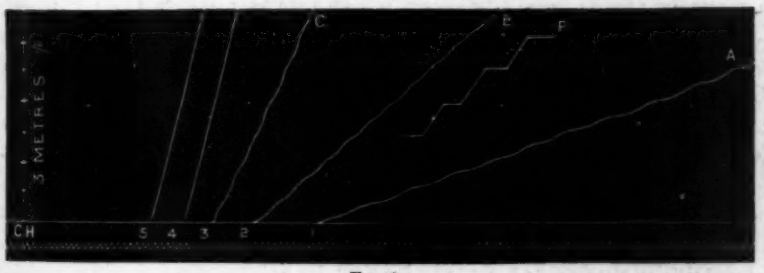


FIG. 2.

The undulations are far greater when the walking is very slow than when it is more rapid. Thus, the motion of the body becomes more uniform by reason of a higher velocity. This is the reverse of what happens with the vertical oscillations of the body, which increase with the velocity of progression and with an increase in the length of the steps.

The number of alternations of motion is double that produced by the movement of a single foot, as shown in Fig. 1. This is readily understood when it is remembered that the two feet, repeating the same acts, give alternately to the body a new impulsion.

To make clear this action we have traced, parallel to the line 2 of Fig. 2, the curves *P*, produced by the motions of the right and of the left foot. These curves, of which one is dotted, and the other full, are at once recognized as similar to those of the line 2 *B*, of Fig. 1. In fact, on observing the superposition of the different parts of these curves with the curve of translation, we see that the body receives an

increase of velocity about the middle of the period when either foot is on the ground. This fact is in harmony with experiments which I have already published.

I will add, in concluding, that one of the most important results obtained by these researches is, the idea which they give of the variability of the movement of translation of the body during walking and running.

In another publication I shall show the applications which can be deduced from these studies to the best utilization of the work of animals.

II. On the Resistance of the Air to the Wings of a Bird during its Flight.—I have presented to the Academy of Sciences a memoir which proves that the wings of a bird, during their downward movements, meet with more resistance from the air when the bird has an horizontal motion of progression than when the bird, depressing his wings with the same velocity, has no horizontal motion of translation.

The explanation of this phenomenon appears to me to be as follows: a wing, or any surface whatever, which moves against the air, meets, at the beginning of its motion, a considerable resistance, on account of the inertia of the air, which resists every displacement; but, little by little, the air yields, and the velocity of its motion gradually increases, until it may equal that of the moving surface which displaces it; this phase of motion having been reached, the resistance diminishes. Finally, when the moving body stops, the moving air tends to continue its journey, and it thereby produces before the moving surface a true *aspiration*, or negative pressure. But a bird, which moves horizontally during the depressions of its wings, acts, during successive instants of that depression, on a series of columns of air over which it passes. From each column it meets with that maximum resistance which the inertia of the air presents at the first instants of the action of the wing. Finally, when the wing has reached the lowest point of its depression, it is not over the mass of air which it has just set in motion, because the motion of translation of the bird continually brings it into regions where the air is at rest. All of these conditions are evidently favorable to flight, since they increase the resistance of the air, which alone furnishes to the bird its support, and the reaction to its moving wing.

To prove the exactness of this theory, I have made certain experiments in which a uniform quantity of work was applied to produce the elevation and depression of the wings of an artificial bird. In some experiments the motions of the wings took place while the machine was stationary; they had a great amplitude of motion. In other experiments we gave the artificial bird a motion of translation, and then we observed that the amplitude of the flaps of the wing dimin-

ished considerably, which fact showed an increase in the resistance of the air.

MM. Planavergne claim the priority of the theoretic idea which I have enunciated, and show, in fact, that they had published, some years since, a memoir, in which this theory is explicitly stated. However, these authors have furnished no experimental demonstrations of their views; consequently, it has appeared to me that it would be interesting to continue the researches which I had begun, and to determine, as accurately as possible, on one hand, the phases and variable conditions of the resistance of the air to a moving body which displaces it with a uniform motion; on the other hand, to find the increase of the resistance of the air under the wings of an apparatus which is transported with determinate velocities.

FIRST SERIES OF EXPERIMENTS.—*Determination of the variable and constant resistances opposed by the air to a moving body which displaces the air with a uniform motion.*

In a solid framework, which glides easily on an horizontal plane, I placed a light screen, with its plane vertical and perpendicular to the direction of its motion. This screen turns around an horizontal axis; and an arm attached to it is charged with an additional weight until perfect equilibrium is established between the arm and the screen itself. This having been done, we have no fear of the inertia of one or another part of the system causing the screen to revolve around its axis at the beginning of its motion of translation. If such a movement of rotation does take place, we must attribute it to the resistance offered by the air.

Behind the screen is placed a little manometric apparatus, which communicates, by means of a tube, with a drum, having a lever resting on its membrane.[1]

FIG. 3.

The apparatus having been thus arranged, we give to it a uniform motion of translation which lasts half or a quarter of a second, and we obtain on a revolving cylinder the above trace of the point of the writing-lever attached to the membrane of the recording drum.

When the screen is at rest, the apparatus traces an horizontal line

[1] For a description of Marey's manometric apparatus, the reader is referred to "Animal Mechanism," published in the "International Scientific Series."

$x\,x$, which corresponds to the zero of pressure on the dynamometer. At the instant of the beginning of the translation the apparatus shows an energetic pressure (a) on the dynamometer; this is the initial variable resistance caused by the inertia of the air against which the screen pushed. Very soon afterward the curve falls, announcing that the resistance of the air has diminished, although the motion of the disk had remained uniform. This is owing to the fact that the air had then partly acquired the motion of the screen. The pressure falls thus to the level b, which marks the constant resistance of the air during the whole remaining period of the translation.

Finally, when the apparatus is suddenly stopped, we see that the trace of the recording-lever is suddenly depressed at the point c; this is due to the variable terminal condition: it consists in the carrying of the screen forward by the column of air already set in motion by it. This negative resistance gradually ceases, and the tracer returns to *zero*.

We were not able to determine with this rough apparatus the absolute value of the resistance of the air corresponding to different instants of uniform translation, but we can readily see that there exist two variable states, of which one precedes and the other follows the constant resistance of the air. The studies of physicists have heretofore been directed only to the determination of the constant resistance corresponding to different velocities.

SECOND SERIES OF EXPERIMENTS.—*Increase of the resistance of the air to the downward movement of the wing of a bird, caused by the horizontal translation of the bird.*

In making the above determination, I have roughly imitated the construction of the bird, by reducing each of the wings to a thin and rigid plane of $\frac{1}{4}$ a metre in length, and $\frac{1}{10}$ a metre in breadth. These two wings are simultaneously depressed by the action of a spring.

A constant amount of work is thus employed for each blow of the wings. The translation of the artificial bird takes place in gliding along an horizontally-stretched iron wire. Two large wheels, one of them furnished with a crank-handle, move an endless cord parallel to the iron wire. The apparatus with wings is attached to this cord, and can thus be moved horizontally with greater or less velocities.

It is now necessary to determine with precision the velocity of translation and the duration of the depression of the wings. The graphic method gives readily these two measurements:

1. *Measurement of the Velocities of Translation of the Apparatus.*—This velocity is evidently the velocity of a point on the endless cord, to which the winged apparatus is attached. This cord passes around a little pulley, whose revolutions are counted and registered on a revolving cylinder by means of a lever which is worked pretty much as the lever in the registering apparatus of Morse's telegraph.

The little pulley which serves to measure the velocities is exactly

$\frac{4}{10}$ of a metre in circumference; one-half of its circumference is covered with a metallic band, the other half is formed of insulating material. Metallic springs press against the periphery of the pulley, so that, while they touch the metallic band, a current from a battery depresses the lever which traces on the revolving cylinder. Hence the lever is depressed, while $\frac{2}{10}$ of a metre of the cord pass, and it is elevated during the next $\frac{2}{10}$ of a metre of passage of the cord. Thus are registered, in an indented line, the velocities of translation of the artificial bird. Evidently the greater the velocity of the bird the greater will be the number of indentations inscribed in a second on the uniformly-revolving cylinder.

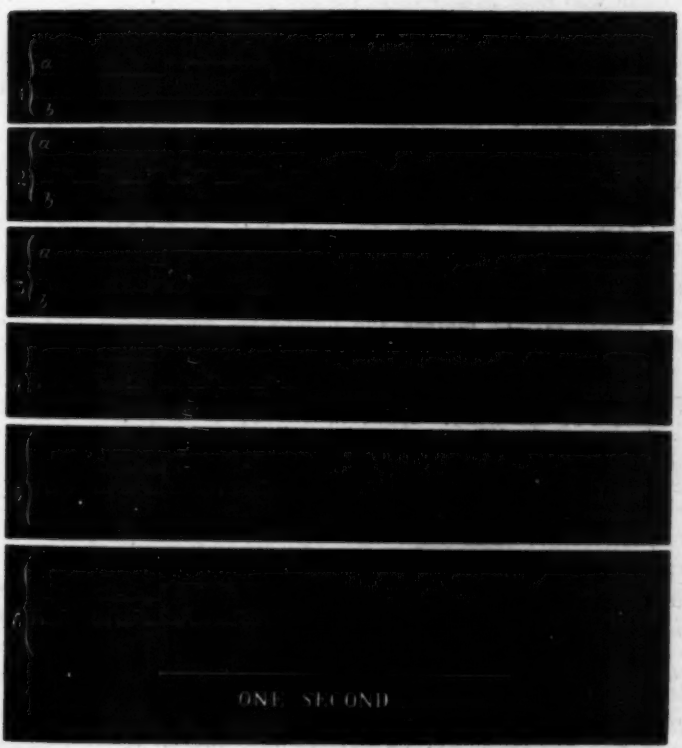


Fig. 4.

2. *Measurement of the Duration of the Depression of the Wings.*—A second electrical recorder, similar to that which registers the turns of the pulley, serves to determine the duration of the depression of the wings. For this purpose it is necessary that, at the beginning of the depression, the current of a battery should be broken, and this action is registered on the revolving cylinder by an indentation in the trace of the writing-lever; also, at the end of the depression of the wings, the current must be closed again, and this instant is likewise registered on the cylinder.

We have thus obtained simultaneously the traces of the velocities of translation of the artificial bird, and the durations of the depressions of its wings, and we have obtained a series of determinations of which the preceding figure (4) furnishes some examples:

Experiment No. 1.—The upward indentation in the line *a* shows the duration of the depression of the wings. Taking that length on the scale of time, we see that the downward movement of the wing lasted less than $\frac{1}{4}$ of a second. In that experiment there was no translation of the bird. The line *b* does not show any indentation.

Experiment No. 2.—The duration of the depression of the wings (line *a*) is already greater; it exceeds one-half of a second. The translation was then nearly three metres per second. We find this by taking in the dividers the length on the line *b* of the $3\frac{1}{2}$ double indentations of the tracer, which show that $3\frac{1}{2}$ times $\frac{4}{10}$ of a metre, or 1.4 metre, have been traversed by the artificial bird. We carry this length to the scale of time, and we find that it is contained about twice in a second. We thus see at once that the duration of the downward motion of the wing increases with the velocity of translation of the artificial bird.

Experiments Nos. 3–6.—In the remaining experiments, proceeding as we have already done, we find that the duration of the depression of the wing increases with the velocity of translation, and that with a velocity of $5\frac{1}{2}$ metres the downward movement of the wing lasts about one second. I have not been able to find the accurate relation between the velocity of translation and the duration of the downward motion of the wing. Experiments made in precisely the same conditions sometimes present slight differences, which are due to the fact that the slightest oscillation of the iron wire, which serves as a support and guide for the artificial bird, slightly changes the durations of the phenomena. From the first series of experiments, it would appear that the duration of the depression of the wing increases in proportion to the velocity of its translation, at least within the limits of the velocities with which I have experimented.

THE RELATIONS OF WOMEN TO THE PROFESSIONS AND SKILLED LABOR.

By ELY VAN DE WARKER, M. D.

THERE are fields of labor in which women have been immemorially active. In all matters relating to the cares of the house and children among the civilized, and, among the barbarous and the lowest strata of life in Europe and elsewhere, field-labor, the care of animals, and the lighter manufactures, are the tasks imposed upon women.

Deduct from this the comparatively small number of women of fashion whose existence is merged into the decorative part of social life, and we have here roughly grouped the lines which have defined the usefulness of women, and which have lain parallel for ages. In the midst of this toiling mass of humanity phenomenal women have appeared—women who have thrown down the dividing lines of prejudice, and created for themselves places among the most celebrated of the other sex. Those who have thus elevated themselves above the mass have demonstrated the capacity of women not only for the highest culture, but also their ability to equal men in the use of faculties which are their most distinguished attributes.

For a generation or more the question of woman's entry into employments deemed man's exclusive province has attracted attention, and raised up for woman a body of aggressive advocates of both sexes, who, by their demands, have provoked some harsh criticism from those who are in no sense the enemies of the intellectual and worldly advancement of women. Woman is now submitting her fitness to find employment, in the learned professions and skilled labor, to the rigid test of actual trial. Will she succeed, or will those of her sex, who achieve success in these fields of labor, be the exception, rather than the rule, in the future as in the past? In order to answer this question, it is my purpose to study woman in this relation, as a gynæcologist,[1] leaving out of consideration the social aspects of the case. One, who has devoted years to the study of women and their diseases, has a right to be heard upon this vital question. I do so the more readily because I know of no gynæcologist who has devoted his special learning to the study of woman's relation to man's work as a means of subsistence and of usefulness. For our purpose, therefore, woman must be scientifically investigated as a means to the accomplishment of certain ends. She must be studied rather in her physical and mental fitness, than in relation to society in her new position. This latter part of the subject belongs to the sociologist.

An examination of the present relation of woman to the other sex will throw light upon the complex problem of her success, in the future, in these fields of usefulness. The women of savage races, except sexually, present but slight differences in physical development and mental character from the males. That they are in base servitude to the other sex is in obedience to the aggressive and belligerent character of all males of the higher order of animals. This has the force of a law. The moral subjection of woman to the other sex is fundamentally a sexual peculiarity. With the slow advent of civilization the differences between the sexes increased. With no lessening of subjugation the capacity of woman for gross labor decreased, and from man's equal, physically, she became only his equal mentally. The chivalry of the middle ages of Europe kept viable the principle

[1] Gynæcology, that branch of pathology which treats of the diseases of women.

of her social equality, which she had wrung from man during the period of classic civilization. Of all things, mediæval woman alone did not retrograde. I believe the modern woman to be the natural outgrowth of the woman of chivalry.

And here let me apply to this question the laws of heredity and sexual selection. These laws touch the human family with as much force as the lower forms of animal life. The fact that man is marked by intellectual power in no way exempts him from the operation of the fundamental laws of biology. Objections which, at the first glance, may appear to be well taken against applying these laws to explain the existing relation of women to the other sex, become of small moment when we consider that, in his sexual relations, man approximates to nearly the level of the lower animals. M. Quetelet, who has made this a special study, remarks as follows: "It is curious to see man, proudly entitling himself King of Nature, and fancying himself controlling all things by his free-will, yet submitting, unknown to himself, more rigorously than any other being in creation, to the laws he is under subjection to."[1] Mr. Buckle, in the introduction to his "History of Civilization," carries the argument of Quetelet to even a greater extent. I think this will satisfy any possible objection to the propriety of applying these laws to the exposition of my subject.

Whatever may be woman's fitness in the future to become man's peer in the professions and skilled labor, there is this fact against her in the present: she is laboring under the accumulated inherited tendency of countless generations. That which had its origin in common with sister animals in physical and moral subjection to the male, has, in spite of the operation of that intellectual force which we see operating so potently at the present day to cast off this subjugation, continued in full force. I can explain this in no other way except as the result of heredity. This position of woman is as clearly a sexual trait as in lower animals. Darwin says that, "as peculiarities often appear in one sex, and become hereditarily attached to that sex, the same fact probably occurs under nature, and, if so, natural selection will be able to modify one sex in its functional relation to the other sex."[2] Dr. Maudsley, in speaking of one relation of woman to man, says: "Through generations her character has been formed with that chief aim (marriage); it has been made feeble by long habits of dependence; by the circumstances of her position, the sexual life has been undesignedly developed at the expense of the intellectual."[3] Mr. Herbert Spencer insists upon this. "Certain powers which mankind have gained in the course of civilization cannot, I think, be accounted for, without admitting the inheritance of acquired modifications."[4]

[1] Popular Science Monthly, vol. ii., p. 46. [2] "The Origin of Species," p. 83.
[3] "The Physiology and Pathology of the Mind," p. 203.
[4] "Principles of Biology," vol. ii., p. 249.

The law of sexual selection also comes in as a factor to account for the present relation of the sexes. Since our modern civilization there have ever been women who aimed to relieve their sex of their dependent relation to man. With this moral force ceaselessly antagonizing the natural relation of the sexes and the forces of heredity, why has it not, in a more marked degree, accomplished the noble purpose at which the more intellectual and stronger minds of the sex have aimed? To offer a reasonable explanation of this, I apply the law of sexual selection. Men and women do not appear to wed out of free choice, but in obedience to law which finds its expression in individual preferences. This, in the human family, may be called sexual selection. Mr. Walker states it in this way: "Love from a man toward a masculine woman would be felt by him as an unnatural association with one of his own sex; and an effeminate man is equally repugnant to woman. In the vital system, the dry seek the humid; the meagre, the plump; the hard, the softer; the rough, the smoother; the warmer, the colder; the dark, the fairer, etc., upon the same principles; and so, also, if here any of the more usual sexual qualities are reversed, the opposite ones will be accepted or sought for."[1] Dr. Ryan, in speaking of selection in relation to marriage, uses nearly identical language.[2]

The annals of literature show that the most eminent of the sex either are unmarried, or are married late in life, and are thus often without issue. The women who intellectually leave their impress upon the age in which they live are the very class to which this law of sexual selection applies. The chances of this order of women leaving daughters who will inherit their superior mental vigor are greatly inferior to those of the average woman. The woman of the average, her mind and ambition being of the measure of the ordinary matters of life, not only seeks a husband by the force of education, but is sought by men. Thus, married early in life, she becomes the source from which the population is recruited. This, in my judgment, is not only a potent cause of the present relation of the sexes, but will serve to explain the chances of women becoming prominent in the professions of the future.

There is another set of laws which apply to this part of the subject. These are the phenomena which are observed in studying human increase, and are called the laws of population. The forces engaged in the evolution of nervous and mental (cerebral) structure are opposed to those necessary to reproduction. Mr. Herbert Spencer expresses it as an antagonism between Individuation and Genesis; and that this antagonism is more marked "where the nervous system is concerned, because of the costliness of nervous structure and function."[3] There is no part of individuation so costly as that of cerebral growth. The more solid expansion of mind is accomplished after general structural

[1] "Intermarriage," by Alexander Walker, 1839, p. 116.
[2] "The Philosophy of Marriage," 1873, p. 70. [3] "Principles of Biology," ii., p. 502.

development has ceased. If mental growth be unduly forced before structural completion, structural and sexual genesis are retarded or impaired. Professional training in women must, therefore, fall within the child-bearing period. In the case of men of great mental activity there is marked impairment of fertility.[1] The cost of reproduction to males is greatly less than to females, and it therefore follows clearly that the prolonged and intensified mental growth, the result of professional training, is to be deducted in full from the sum of the forces necessary to reproduction. M. Quetelet cannot doubt the influence of professional life upon fertility.[2] Mr. Herbert Spencer says, "that absolute or relative infertility is generally produced in women by mental labor carried to excess, is more clearly shown. . . . This diminution of reproductive power is not shown only by the greater frequency of absolute sterility, nor is it shown only in the earlier cessation of child-bearing, but it is also shown in the very frequent inability of such women to suckle their infants. In its full sense, the reproductive power means the power to bear a well-developed infant, and to supply that infant with the natural food for the natural period." [3]

Even were it not that absolute and relative infertility are against the woman undergoing severe mental discipline having children to inherit her improved cerebral evolution, and in favor of the average or inferior woman, still the very condition of this mental discipline, if the woman is preparing for the professions or skilled labor, involves a postponement of marriage to a period when, in the mass of wives, fecundity has received a permanent check. The average individual wife shows a degree of fecundity which, at the age of twenty-five, diminishes,[4] and this is the period at which the professional woman is prepared to enter upon her business career. The opinion of Mr. Sadler, that delayed marriages developed a degree of fertility in women which compensated for the loss of fecundity consequent upon the delay, is completely overthrown by the tables of Dr. Duncan.[5] If women are to enter the learned professions and skilled labors, they must be devoting themselves to training at a period of their lives when the mass of women are wives—mothers. I think that it must be conceded as a fact that, to contract matrimony during this period of mental and bodily training, would totally defeat the selected life-work of the woman. The desire to become the co-worker with man upon the highest level of man's work belongs only to superior women; if, in addition to this innate superiority, we add that acquired from increased cerebral development, the law of heredity would tend to continually en-

[1] "Principles of Biology," ii., p. 487.

[2] A Treatise on "Man and the Development of his Faculties." Translation. Edinburgh, 1842, p. 21.

[3] *Loc. cit.*, p. 486.

[4] Dr. Matthews Duncan, "Fecundity, Fertility, Sterility, and Allied Topics," Edinburgh, p. 43.

[5] Sadler, "The Law of Population," ii., p. 279.

large the number of women fitted by inherited traits to occupy this advanced field. But we have shown that the laws of sexual selection and of population are entirely opposed to the increase of women thus favored, and in favor of the average woman by a large per centum.

I am inclined to regard all forces which have hitherto acted, and yet continue to act, upon the great mass of humanity in the creation of sentiments common to the majority upon a given subject, as acting with the force of a law. I conceive, therefore, that there is yet another law which explains, and tends to perpetuate, the present relation of women to the other sex and to society. This is the law of public opinion. The exponents of public opinion upon this subject are the women themselves. I do not think any one will controvert me when I assert that a vast majority of women are opposed to their own sex entering the professions. One would naturally suppose that, in the matter of religion, a woman's opinion is as good as a man's; that, with equal learning and experience, a woman is as competent to discharge pastoral duties as a man (I am assuming the physical equality of the sexes); and yet you may count upon the fingers of one hand the number of pulpits filled successfully by women in this great country. In this country women are free to enter the medical profession; but, with about as many exceptions as that of women filling pulpits, they are gaining but a precarious and scanty support. Now, in both the professions named, women are retarded by the force of opinion of their own sex. In all social questions, women wield a great influence. In these matters they are the throne, and the power behind the throne. In Protestant congregations, if women were a unit in favor of women preaching, women would preach in a fair proportion of church organizations. If a woman made a free choice of a physician of her own sex, there is scarce a household in which she would be denied her choice. Women seem to lack confidence in their own sex in this position. In the desperate diseases peculiar to women, the sorely afflicted ones seek the medical man instead of the medical woman. The future has yet to produce the anomaly of the female ovariotomist. In the literature of medicine there has been but one Boivin, and but one La Chapelle. The reliance upon man in moments of bodily peril is easily explained; it is an inherited trait, strengthened by education.

I have said enough to explain philosophically the present relation of women to the other sex, and to society. It is this relation which has, in the past, regulated woman's admission into the professions and skilled labor. But we have now to accept the fact that women have entered the professions to contend with man in the struggle for existence. In this struggle, I presume, women expect no favors. In this new field of contest all they ought to ask for is a fair chance to win—the same chances man must take. But, in view of her present relation, and the radical physical differences between the sexes, have they a fair chance, and can they take the chances of man and reach his level

in the professions? This is the question I shall endeavor honestly to solve.

Mentally, I believe woman to be the peer of man; that there is nothing about law, medicine, or theology, that woman cannot learn as well as he; that her mental difference is a sexual difference, just as her bodily differences are sexual. To the question of her mental fitness for this work, other than sexual, I shall give no attention. The physical difference between the sexes will form the first part of our study of woman's future relation to the professions, and brings us naturally to the discussion of the law of anatomical development as modified by sex. In the July number of THE POPULAR SCIENCE MONTHLY, in an article entitled the "Genesis of Woman," I endeavored to assign a proper value to the functional development of woman. In that paper, the anatomical development of woman was studied with reference to her functional genesis; but here it must be studied with reference to her fitness for competition with the opposite sex in the struggle for subsistence. Now, there are certain skilled labors which belong to man by virtue of his superior strength. The anatomical peculiarities of woman do not need to be contrasted with man's in reference to this class of labor, and women in the lower walks of life have demonstrated their ability for severe bodily toil. But the intellectual work—to which I am mainly directing my attention—to which women are reaching, implies that the candidates possess the delicate structural development, the inherited result of civilizing forces. We can draw no inference, therefore, from that fact that women, less exposed to these forces, and more nearly approximating man in physical strength, fully equal him in the value of their labor. If we examine some of the lighter and more delicate forms of skilled labor, such as we would naturally conclude were peculiarly fitted for the delicate and nice touch of women, we find them in the hands of men almost exclusively. The question of mental fitness must be excluded, for mentally they are as competent as man is to acquire and practise these arts. I think it can be shown that anatomical unfitness, aside from her inclination, is the obstacle. In the manufacture of instruments involving great delicacy, and, until the introduction of machinery, in the manufacture of watches, as a class women were excluded. While not involving any great muscular outlay, these samples of skilled labor demand great delicacy of educated touch. While we must make great allowances, as an anatomical factor, for the advantage to men so employed of the inheritance of mechanical taste and skill from fathers, oftentimes so employed for generations, yet it is a common error to suppose that employments which involve delicacy of manipulation do not require strength, as great in degree as, but differing in kind from, that demanded by more hardy labor. Educated, coördinate muscular movements depend more than any other upon strength and certainty of muscular contraction. In no particular, aside from sexual differ-

ences, is the male skeleton so greatly different from the female as in the irregularities and asperities of the bones for the attachment of the muscles. While in man they form a marked feature of his bony structure, in well-formed females they present but a comparatively scanty development. It is true that both muscles and osseous irregularities, for their origin and insertion, may be developed by training, yet woman, as at present related to the other sex, has not only to acquire his strength, by a course of extra training, but she must equal him in skill, if she is to prove a successful competitor for his place. These comparisons between the physical strength of the sexes would be altogether unfair, were it not for the fact that they are invited by the position women have elected for themselves, and are essential in giving an opinion of woman's chances of success.

The fact that those employments are chosen by women which permit a sitting position is significant in this relation. Woman is badly constructed for the purpose of standing eight or ten hours upon her feet. I do not intend to bring into evidence the peculiar position and nature of the organs contained within the pelvis, but to call attention to the peculiar structure of the knee, and the shallowness of the pelvis, and the delicate nature of the foot as part of a sustaining column. The knee-joint of woman is a sexual characteristic. Viewed in front and extended, the joint in but a slight degree interrupts the gradual taper of the thigh into the leg. Viewed in a semi-flexed position, the joint forms a smooth, ovate spheroid. The reason of this lies in the smallness of the patella in front, and the narrowness of the articular surfaces of the tibia and femur, and which in man form the lateral prominences, and thus is much more perfect as part of a sustaining column than that of woman. The muscles which keep the body fixed upon the thighs in the erect position labor under the disadvantage of shortness of purchase, owing to the short distance—compared to that of man—between the crest of the ilium and the great trochanter of the femur, thus giving to man a much longer purchase in the leverage existing between the trunk and extremities. Comparatively, the foot is less able to sustain weight than that of man, owing to its shortness and the more delicate structure of the tarsus and metatarsus. I do not think there can be any doubt that women have instinctively avoided some of the skilled labors on anatomical peculiarities.

The question is in order, To what extent will these anatomical disadvantages act as a bar to her future progress? The present skill of man is the sum of functional and organic evolutions attendant upon countless generations. Women, during this period, have also been passing through the same series of evolutions. But the sum attained by women, although equaling that reached by men in sexual value, differs totally in kind. Under the condition of the sexes we are studying, these lines of evolution must maintain a perfect parallelism in order to secure equality in the sexes. Physically and

intellectually, the two sexes must move side by side to future evolution. At present, woman must unlearn part of her innate education, and acquire some of that of man; otherwise she cannot equal him in value of skilled labor. Woman must be content to grow up, to evolve, generation by generation, to a position from which she can compete with man in the fields of labor. I believe this condition of things not only can be realized, but in the course of generations will be reached. When we reflect that the present impaired value, in a labor point of view, of educated women is but the result of civilizing forces and the increment of inherited traits, and that women in lower or savage life fairly equal men in the value of their muscular development, we have every reason for this belief. The reader must bear in mind that I am treating of the sex as a unit. Individual exceptions, which always have occurred, and, however prolonged the existing relation of the sexes may be, probably will ever occur, do not apply as negative facts to my argument. The laws of sexual selection, of population, and of heredity, will oppose the advance of women, other than in this exceptional way. But there exists in society a force which is tending to the parallel evolution of the sexes. This force lies in the large excess of females in the adult population of many countries. Stern necessity will force—if this condition of affairs continues in the future—a large percentage of this excess to compete with man in the professions and skilled labor. Many of these trained women will marry and have children, and thus form nuclei, divergent lines from which will extend into posterity, ever adding increment upon increment to the forces which tend to parallelism in the evolution of the sexes.

The purely sexual anatomical differences I shall say nothing about; but the functional resultants of these anatomical conditions, both mentally and physically, must be studied with reference to their effect upon woman's chances of success. If we examine carefully the mental action of women, we perceive in it an undercurrent of sex. As there are organs which characterize sex, so also is there a sexual cerebration. We know from experience that this unconscious dominance of sex in cerebration in no way interferes with high culture, and the exercise of the best qualities of mind. It is a normal condition of mental action in women, but its existence implies conditions which may at any moment render mental action abnormal. Take the emotions, for instance, the undue exercise of which are so liable to assume morbid proportions, as in hysteria. Here sex, when it asserts itself unduly, obtrudes inharmoniously into what otherwise would be healthy mental action. It is in this class of mental actions, termed the emotions, that the mind of woman forms part of the sexual cycle. Some of these actions are so elementary that they are called instincts. The maternal affection, and also love, partakes of this instinctive character. The exercise of the sympathies is more general and active in women than in men. This is one of the features which give such beauty to

the character of women, and is not the result of education. Mungo Park tells us that, when sick and thirsting, and maltreated by the natives of Africa, the women of the savage tribe visited him and supplied his wants. I will give one instance, which is a type of character, and shows how sympathy and natural feeling may interfere with professional advancement.

The wife of a practising physician, being of a scientific tendency of mind, acquired a theoretical knowledge of her husband's profession. The husband died, and left the widow poor and with several children, some of them so young as to demand much of her time and thought. She continued the study of medicine with the design of making it the means of support for herself and children. To this end she attended lectures at a woman's medical college. Before she obtained her diploma, an old, superannuated Presbyterian clergyman excited her sympathy by his forlornness. She gave him a home in a very womanly way—she made him her husband. Here was a double burden—an old man, and little children. This physician, although laden with her great, womanly heart, was prosperous in a small way. She secured the position of house-physician in the hospital connected with the college, with a small salary, and with sufficient time to attend to private patients. Her pecuniary prospects were better than those of young physicians of the other sex. The husband soon died. At this point in the history occurred an incident which seems to me to be phenomenal, and yet is typical. A second old clergyman, equally forlorn and wretched as the first, accepted the charity of this woman by becoming her husband. Her practice slowly increased; her children were well clad and well educated. A daughter married, and moved, with her husband, to a distant city. A son studied medicine, and the last husband died. The next act in this singular history reveals an intensity of maternal feeling entirely opposed to a business success in a difficult profession. Gifted with a fine mind, as thoroughly educated in her profession as the majority of medical men, with good health, and having reached that time of life when she was functionally at rest, and with every encouragement to remain at her post, yet she made a better mother than doctor. She resigned her position in the hospital; abandoned her private practice; and moved to the city in which her daughter resided, in order to be near her child and grandchildren—and there, in a strange community, recommenced the difficult occupation of a female physician. This history is truly a physiological study, and reveals the intensity of feeling which may exist in all women upon subjects which lie near the heart.

The common standard of professional success is a pecuniary one. Public opinion will apply this standard to women as rigidly as it does to men. It is a common experience to meet men of the highest mental training in their professions, yet who fail completely in a business sense, owing to idiosyncrasies of mind. In this way the sympathies

and affections to which women are prone will retard them in their pursuit of a profession as a business investment. In the medical profession, to which women have a special leaning, the constant witnessing of human suffering and misery will call into play emotions which will interfere with the calm and deliberate study of each case, which its rational treatment will demand. The same objection applies to men; the medical man is rarely to be trusted to treat a difficult case in his wife, or child, or himself.

There is one fact in woman's functional life which is of vast importance to the subject of this paper, and which I refer to with great reluctance. This fact is ovulation. The mental reaction of this function is oftentimes of such a character as, for the time, to totally incapacitate for professional or other mental work. As this paper is written solely with the view of arriving at the truth in a matter of great practical importance, I must let this serve as my apology for referring plainly to this subject; and this importance requires that I let others, who are acknowledged authorities in gynæcology, speak for me.

Dr. Robert Barnes, of London, the author of the latest work upon gynæcology, uses the following unequivocal language: "The mind is always more or less disturbed. Perception, or at least the faculty of rightly interpreting perceptions, is disordered. Excitement to the point of passing delirium is not uncommon. Irritability of temper, disposition to distort the most ordinary and best-meaning acts or words of surrounding persons, afflict the patient, who is conscious of her unreason, and perplex her friends, until they have learned to understand these recurring outbursts. . . . Not even the best-educated women are all free from these mental disorders. Indeed, the more preponderant the nervous element, the greater is the liability to the invasion. Women of coarser mould, who labor with their hands, especially in out-door occupations, are far less subject to these nervous complications. If they are less frequently observed, if they less frequently drive refined women to acts of flagrant extravagance, it is because education lends strength to the innate sense of decorum, and enables them to control their dangerous thoughts, or to conceal them until they have passed away."[1] Another of the accidents attendant upon ovulation is hysteria. Dr. Tilt defines it as a disease peculiar to women during the reproductive period of life, and is often known to return at each period of ovulation.[2] This function is constantly liable to accidents. Speaking of the mental effects of æmenorrhœa, a disease to which every woman is liable who follows an intellectually rather than a physically active life, Sir J. Y. Simpson says that she becomes "subject to fits of excitement which come on most frequently at a menstrual period, and which usually assume an hysterical form, but

[1] "A Clinical History of the Medical and Surgical Diseases of Women," p. 162.

[2] "Diseases of Menstruation and Ovarian Inflammation," p. 129.

are, at times, almost maniacal in character."[1] I shall make but one other quotation, and I am glad to say that it bears directly and practically upon this matter. Dr. H. R. Storer, of Boston, is reported to have spoken as follows in a debate at the Gynæcological Society of Boston, May, 1870: "In the present excited state of public opinion, it were foolish, and at the same time unkind, to object to female physicians upon any untenable grounds; and he frankly stated that the arguments that physicians had usually employed, when discussing this subject, were, almost without exception, untenable. Some of the women who were desirous of practising physic and surgery were just as well educated for the work, had just as much inclination for it, and were as unflinching in the presence of suffering, or at the sight of blood, as were many male practitioners. They had a right to demand an acknowledgment that, in these respects, they were as competent to practise as are a large proportion of ourselves. There is, however, one point, and it is upon this that the whole question must turn, that has till now almost wholly been lost sight of: and this is the fact that, like the rest of their sex, lady doctors, until they are practically old women, regularly menstruate, and are therefore subject to those alternations of mental condition, observable in every woman under these circumstances, which so universally affect, temporarily, their faculties of reason and judgment. That these faculties are thus affected at the times referred to is universally acknowledged."[2]

Many other authors may be cited to the same effect; but these are sufficient to render evident the possibilities of danger, if not of disaster, to women subject to the ceaseless calls of professional life.

Among popular writers upon this subject, the matter of wifehood or motherhood has been treated as if, were woman willing to sacrifice some of her traditional feeling, and voluntary likings for the other sex, she might cast off the fetters of these honorable conditions, and move on untrammeled to the study and practice of a profession. We have been studying woman, in her relation to the subject of this paper, as a sexual being; and, if we continue the study in the same direction, we must arrive at the conclusion that marriage is not an optional matter with her. On the contrary, it is a prime necessity to her normal, physical, and intellectual life. There is an undercurrent of impulse impelling every healthy woman to marry. That this is a law of her sexual being we know by the positive evidence of medical men and others. We also know that the married woman exerts a more marked influence upon men, and society in general, than the celibate. There is also, among married women, a more perfect equilibrium between the intellectual, physical, and sexual forces; and yet, necessary as marriage is for woman, in the present relation of the sexes, it must in every way impair her prospects of success in professional work.

[1] "Diseases of Women," p. 617.

[2] "The Journal of the Gynæcological Society of Boston," vol. ii., p. 267.

The effect of celibacy upon women has often elicited the remarks of gynæcologists. Dr. Tilt says of marriage: "It is easier to prove the benefits of marriage than to measure accurately the evils of celibacy, which I believe to be a fruitful source of uterine disease. The sexual instinct is a healthy impulse, claiming satisfaction as a natural right."[1] Again: "An enlarged field of observation convinces me that the profession has not in any wise exaggerated the influence of marriage on women, and that its dangers are infinitesimal as compared with those of celibacy."[2] Nearly every treatise upon gynæcology may be quoted to establish the same fact. It is upon the mind of woman that the defeated sexuality acts reflexly in a morbid manner. Dr. Maudsley, who has had abundant opportunities for observation, says: "The sexual passion is one of the strongest in Nature, and as soon as it comes into activity it declares its influence on every pulse of organic life, revolutionizing the entire nature, conscious and unconscious; when, therefore, the means of its gratification entirely fail, and when there is no vicarious outlet for its energy, the whole system feels the effects, and exhibits them in restlessness and irritability, in a morbid self-feeling taking a variety of forms."[3] While it is true that the engrossing cares of professional life, or of a skilled labor, will serve as a partial "vicarious outlet for its energy," in contrast to an idle life, yet this will in no manner act as a substitute for the natural expression of this physiological want. Its constant suppression will tinge the thought and manner of the woman. This is not an unreasonable statement, when we reflect that bodily derangements, not at all serious, will often account for changes in the mind and manner, as well as for the entire mental habit of men otherwise strong. If we contrast her with man in this respect, the chances are infinitely against woman in professional life. The penalty of sex is an episode in man's life. The tribute to his sexuality once paid, he is practically unsexed, and the trained intellectual man moves among women and men with scarcely more than a consciousness of his reproductive faculty. But sex in woman is a living presence. From the age of fifteen to that of forty-five, her life is crowded with startling physiological acts. Ovulation, impregnation, conception, gestation, parturition, lactation, and the menopause, contend with each other for supremacy—each act a mystery; each attended with its peculiar peril; and most of them evoking in its behalf the highest efforts of which her physical organization is capable. It will demand genius indeed to enable woman to rival man in the field of labor, and, at the same time, contend with the *inexorable law of reproduction.*

Having shown that women are not free agents in the matter of marriage, but do so in obedience to a primal law of their sexual life, we will next consider what are the chances for the married women in

[1] "Uterine Therapeutics," p. 224. [2] *Loc. cit.*, p. 127.

[3] "The Physiology and Pathology of the Mind," p. 205.

professional life. In a physiological study such as this, we will not concern ourselves with the social obstacles a married woman must encounter. We have a right to consider every woman who has a husband as either a mother, or liable to become one. Any attempt on the part of a wife to avoid children in order to free herself of that obstacle to professional life would be attended with consequences to her mental and physical health which would seriously impair her usefulness.[1] The end and aim of woman's sexual life is perfected by maternity. It broadens and elevates her intellectually and physically. The influence over society reached by wives-mothers is a natural outcome of the stimulus of maternity. The maternal instinct, which lies dormant in the nature of every woman, awakens her mental being into increased activity the moment it is called into life. I think that it is for this reason that frail women, with no knowledge of life, when widowed, often succeed in keeping their families together and providing for them. With the woman who is constantly liable to the demands of a profession, or skilled labor, the maternal affection, anxiety, or care, may intrude at moments when her occupation will demand her highest mental efforts. The manual labor of rearing children the professional woman may delegate to others, but the ceaseless love, care, and forethought, so beautiful in a mother's love, the true woman must assume herself. Physically, children are necessary to the married woman. The sterile wife is constantly exposed to diseases that the fecund wife is comparatively exempt from. The sterile wife is not a normal woman, and sooner or later this physical abnormality finds expression in intellectual peculiarities. Not upon the mind alone, but upon the body as well, does motherhood have a maturing influence. Gestation is nearly the completion of the sexual function. The process involves increase in the size of the heart, and in the volume and strength of nearly all the muscles of the body.[2] It is evident from this that gestation is not only a functional completion, but it is necessary to structural maturity, and to me it seems a natural corollary that it has an equal effect in increasing mental vigor. Having shown that marriage is in obedience to a physiological law, and that maternity is necessary to insure mental and bodily health in the mass of women, it is proper for us to ascertain if the last of these conditions—gestation—is not of itself, physically and mentally, an obstacle to professional life in women. The physical incapacity is too evident to need any comment.

Mentally, the changes undergone are most singular and multiform, and operate upon the cultivated and ignorant alike. Dr. Montgomery, speaking of the nervous irritability of pregnancy, says: "It displays itself under a great variety of forms and circumstances, render-

[1] Bourgeois, "The Passions in their Relation to Health and Disease," p. 162, *et seq.*

[2] Dr. Alfred Wiltshire, "On the Influence of Childbearing on the Muscular Development of Women." Transactions of the Edinburgh Obstetrical Society, vol. ii., p. 237.

ing the female much more excitable and more easily affected by external agencies; especially those which suddenly produce strong mental or moral emotions. Hence the importance of preventing, as far as possible, pregnant women from being exposed to causes likely to distress, or otherwise strongly impress their minds."[1] These objective mental conditions described by the author must not be regarded as exceptional; on the contrary, they are classed among the usual symptoms of that condition. Still more marked mental disturbances may occur and are not rare, as in the following quotation from Dr. Storer: "Strange appetites, or longings, as they are called, and antipathies, are well known as frequent attendants on pregnancy in many persons."[2] And further: "The evidence that I have now presented proves that the state of pregnancy is one subject to grave mental and physical derangements, giving rise to serious anxieties, and requiring judicious treatment."[3] These mental effects are of minor importance in the relation we are studying, when we consider the fact that absolute insanity may be an accompaniment of either gestation, or follow parturition. Dr. Maudsley refers to this as explaining the excess of female insane over that of the other sex.[4]

Dr. Forbes Winslow draws a startling picture of this catastrophe: "When, after numerous struggles to repress them, the propensities excited into such fearful and almost supernatural activity by ovarian irritation burst forth beyond all control, and the pet of the family is seen to be the opposite, morally, in every respect to what she had been—irreligious, selfish, slanderous, false, malicious, devoid of affection, thievish in a thousand petty ways, bold, maybe erotic, self-willed and quarrelsome; and if the case be not rightly understood, great and often irreparable mischief is done to correct what seems to be vice, but is really insanity."[5]

We have but one other sexual accident to consider which may act as a bar to woman's progress in the professions. These accidents are incident to the climacteric period of life. This period includes the years between forty and fifty, and, judging from men, a professional woman ought then to be most actively engaged in her occupation. It is during the functional changes then taking place that women are exposed again to the dangers which attend the advent of puberty. It is the second and last crisis in the functional life of woman. We will let the mere bodily diseases of this period pass unnoticed, and refer to those of cerebral origin, as mind forms the working organ of the professional woman. Dr. Bedford regards the varieties of nervous irritation peculiar to this period as "beyond calculation."[6] In fact, it is upon

[1] "Signs and Symptoms of Pregnancy," p. 17.
[2] "The Causation, Course, and Treatment of Reflex Insanity in Women," p. 139.
[3] Ibid., p. 148. [4] *Op. cit.*, p. 207.
[5] *Journal of Psychological Medicine*, January, 1851, p. 43.
[6] "Clinical Lectures on Diseases of Women and Children," p. 374.

the nervous system principally that the cessation of ovarian function acts reflexly in an abnormal manner. Thus, 500 women divided among them 1,261 forms of cerebral disease, confirming the general belief in the frequency of cerebral diseases at the change of life.[1] The liability to insanity at this period is greater in women than in men. Leaving out of consideration such an extreme result as insanity, yet the lighter shades of nervous derangement which would entirely unfit a woman for healthy mental work are so multiform, and to which men are in no way exposed, that it is evident that at this period woman would encounter some of the most stubborn barriers to her success in professional life. The professions, in giving undue employment to the mind, would greatly predispose a woman so employed to nervous disease at the change of life. Her very employment, to which many are working their way so bravely, is almost sure to entail suffering and danger at a period when educated and refined women, more than any others, require mental and bodily repose, and which the nature of their employment forbids. With this brief notice of this important crisis in the life of woman I shall close this part of the subject, and simply offer, in conclusion, a summary in the form of a

RECAPITULATION.

The moral subjection of woman to man is a sexual peculiarity.

This has been perpetuated and intensified in the human family by the law of heredity.

That the tendency of civilization and education to antagonize this subjection of the sex is neutralized by the law of sexual selection.

That the chances of intellectually active women leaving issue to inherit their improved mental character are greatly in favor of the average woman in obedience to the law of population.

That women are retarded in their advancement to professional work by public opinion.

That women have unconsciously avoided some of the skilled labors by reason of anatomical unfitness, and which will be operative in the future.

That sexual cerebration is liable to assume undue prominence in the cultivated and ignorant alike, and thus unfit her for professional mental work.

That women marry in obedience to a sexual law, and not from choice; and that marriage, in the present relation of the sexes, is an obstacle to professional success.

That if women remain single, in order to enhance their professional success, celibacy entails many physical and mental evils, which will impair their value in professional life.

That ovulation may, in many cases, be the cause of mental excitement, or require strong efforts of repression, which would unfit her, for the time, for professional work.

[1] Dr. Tilt, "The Change of Life in Health and Disease," pp. 164, 185.

That children are necessary to the mental and physical health of married women, but that maternity is unfavorable to success in skilled labor or the professions.

That the functions of gestation and parturition are very liable to be attended with mental disturbances which would totally defeat a professional career.

That the change of life is a critical period, prone to be attended with mental and bodily infirmities, unfitting a woman for professional work.

With such possibilities before her, and such necessities urging her, what chance has woman of successfully competing with men of mediocrity in professional life, or in skilled labor? It must be the intent of every woman who essays a professional life to do man's work as well as man can do it, and to secure man's reward for such well-doing. But, I cannot avoid the conclusion that, in the present relation of the sexes, such a standard is impossible of attainment. Whether the conditions which have created and continued the present relation of the sexes will operate as potently in the future as in the past, is a difficult question to answer. I have already said that, in my opinion, there now exist in society forces which will tend to modify the dependence of women. Prominent among these is the persistence with which women are working their way to new relations, which, if continued, will certainly bear fruit, and evoke in its behalf the law of heredity, which is now opposed to them. If we look upon society in a scientific spirit, we must recognize it as a field in which antagonizing forces are contending. This effort of woman to invade all the higher forms of labor is a force battling with the established order of sexual relation. The inertia which it encounters is the universal attendant of established facts in society. If this effort of woman is continued into coming generations, I have no doubt but her relations to society and labor will be in many respects modified. But I believe a long series of generations must pass before women can equal the labor value of men in the professions.

REASON AGAINST ROUTINE IN THE STUDY OF LANGUAGE.

FROM THE FRENCH OF CLAUDE MARCEL.

Part II.

I. GRAMMAR.—Can the exercises of the university and of our lyceums give to pupils the advantages they ought to expect in linguistic study? No, a hundred times no! There is little in these exercises that addresses the judgment, or that will be useful in the course of life. The pupils never read authors, they translate them before they comprehend them; or else translate them in fragments—two infallible means of never knowing them.

The first book put into their hands is a grammar, the most abstract, the most fatiguing, the most unintelligible book that can be imagined, while, at the same time, it is the most useless at the beginning of the study, when the pupil has not yet gained a knowledge of the facts on which it rests. Contrary to reason, the grammar treats of words that occupy the attention before the ideas they represent. It is but a collection of rules and definitions, more or less obscure, incomprehensible, and inapplicable, as preparatory study.

If, as reason teaches, the art of reading is the first object of this study, grammar is not the least help in securing this end: it does not give the meaning of phrases and words, the only difficulty in beginning to read a foreign language. The thought of the author, in other words, the translation that interprets it, not the grammatical condition of the words, should be the first object of consideration with the beginner. He might know the grammar from beginning to end without understanding a word of the language. It certainly is not the art of reading, and cannot be the introduction to the study of language. The method that gives priority to the arts of speaking and writing has recourse to grammar at first; for, in default of example, rules are the only guides of study. But in reading, as in listening, the phrase presents itself, as a whole, to the mind; rules which coördinate the composition have no force until its parts are understood. It is, in fact, by language that we comprehend the grammar, not by grammar that we comprehend language.

Admitting that grammar teaches to speak and write correctly, that is not teaching to *speak* and *write*, but only to do them correctly; in other words, to avoid or correct errors that might glide into expression and thought. It is, therefore, necessary to begin by speaking or by writing, to get any advantage from the rules of grammar.

In beginning with grammar, children do not see its utility, and are disgusted. They are neither interested nor profited, because they do not give it their attention. On the contrary, what interest grammar awakens, when, in place of presenting it dead, so to say, in an abstract manner, it is made to arise from the phraseology, rousing the curiosity, leading to the generalization of facts through observation and reflection, and so opening a vast field to the intellect!

Of all the means that tradition and routine have established in teaching language, grammar is, perhaps, the most prejudicial in retarding practical knowledge. By a deplorable violation of the laws of Nature, substituting synthesis for analysis, putting precept before example, theory before practice, grammar is made the base of language. The minds of children are loaded with principles and theories, roots of words and their etymologies, as if they were all to be philologists and teachers of languages.

Grammar indicates, only in a limited way, the received usage; there are many idiomatic expressions concerning which it is no help.

It does not explain the value of words, nor their proper use, and adds little to our vocabulary, though an abundance of words is indispensable to correct speaking and writing. It teaches neither pronunciation nor accent, nor the orthography of the variable parts of words, nor their diverse meanings, nor the difference of signification between words improperly called synonyms, nor the propriety of figurative language, nor any of those delicacies of expression which constitute the genius of a language, and characterize a clear, elegant, and correct style. So grammarians, who devote their lives to the rules of language, are scarcely famous for their style. I do not know of one who has ever distinguished himself as an orator or writer. On the contrary, the greatest writers, such as Corneille, Pascal, Molière, La Fontaine, and others, owe nothing to grammar; it did not exist in their time. The same is true of Homer, Thucydides, Virgil, Cicero, Dante, Petrarch, Milton, and Shakespeare. Grammar, then, is not the art of speaking and writing correctly, and still less is it the art of reading, by which we ought to commence the study of language. "I should be glad," said Locke, "if I could be shown the language that could be learned by the rules of grammar." "A century of theory," said Lemare, "will not advance us a step in the knowledge of language." "It is the grossest mistake," said Condillac, "to commence with rules."

M. Jules Simon, in suppressing the mnemonic lessons of grammar, has rendered a true service to linguistic teaching. Rules, no matter what, or how many, confided to the memory, will never instruct a man, nor will they give habits of patient observation. A man might learn by heart all the laws that govern the sciences, and hold them as certainties, without at all developing his intelligence. If we could introduce into a man's head, without effort on his part, a knowledge of all the facts and results of scientific research, he would be in reality less capable than he who had learned, by a rational method, to work out a sum in the rule of three.

As a corollary of the grammar, children make grammatical analyses which draw their attention to the classification and function of words, but do not in any way enable them to understand an author, or express their own ideas, or exercise the judgment. These analyses teach grammar, not language. The man without the least ability for making them understands what he says, or what is said to him, as well as the most profound grammarian.

II. Of Themes.—The theme, auxiliary to the grammar, and the favorite exercise of the university, is no better than the grammar to teach reading. The understanding of a written text does not imply the power to write. Reason requires that the learner read before writing, and so secure the means of knowing good usage and imitating the style of great writers. The particular figurative words of a language that have no French equivalents, and the idioms of frequent use in conversation, are quite outside of themes which simply exemplify the

rules of grammar. They give some exceptions, but few in comparison to the whole number. The verb *faire*, to make, for example, is translated into English in more than a thousand different ways, in as many idiomatic forms of expression. Frequent intercourse with foreigners, or else assiduous reading of good writers, can alone make these expressions familiar. Besides, it is contrary to reason to oblige children to compose in a language they will never, perhaps, have occasion to write, when comparatively so little effort is made to acquire the talent in their own language where it is so useful in every moment of life.

Sources of error and *ennui* as are these sterile tasks, they seem calculated to mislead, rather than to form, the judgment. Like most routine processes, they appear to have been invented to give masters business in correcting errors that would have been impossible by processes conformed to reason.

Themes are condemned by all writers upon linguistic study. Rollin, timid as he was in reforms of teaching, said: "To compose well in Latin, one must know the turn, the phrases, the rules of that language, and have accumulated a considerable number of words, the force of which he feels, and of which he can make a proper application. All this can be done only in translating authors who are living dictionaries and speaking grammars; by which he learns the force and true use of words, of phrases, and of rules of syntax. To do this, themes must be absolutely discarded, as they only torment children by painful and useless labor, and give them disgust for a study which brings them only reprimands and punishments." The intimate relation between thought and style, in composition, exercises the highest reason only when the language is the direct and spontaneous expression of ideas, as it is in the mother-tongue; but, in writing a theme, the student is not occupied by the thought; his attention is only directed to the words—their orthography, their concordance, and their arrangement, conformably to the rules he has under his eyes, or that he has previously studied. However, the university makes the knowledge of a language consist in the art of writing it, the least useful part, the least interesting, and the least calculated to exercise the intelligence under the conditions in which it is done. The translation of a native author into a foreign language, which is frequently imposed upon beginners, surpasses in absurdity the method of syntactic themes. It demands of the inventive faculties that which depends solely on imitation. How, without having heard a language, or read it much, without knowing the true value of its words, without knowing what approved usage allows or condemns, and in complete ignorance of its idiomatic forms of expression—how, I say, can a pupil form correct phrases? Knowing neither the different acceptation of words, nor the shades of meaning which distinguish synonymous words, nor the constructions peculiar to the genius of the language, the student never

has occasion to compare, to judge knowingly; he chooses neither the proper word nor the most suitable form, and cannot aim at clearness, force, or elegance of style. The longer he perseveres in this thankless work, the less chance has he of ever writing the foreign language in its idiomatic purity. Latin compositions of this kind have been kept up to the present time, because they were not controlled by critics of the time of Augustus.

Translation into the national language presents numerous difficulties to those who best understand it. Lamartine called a translation the most difficult of all books to make; and we set young children at this work in an idiom which is nearly unknown to them. As well force them to walk with the head in a sack. It is a real tyranny.

The hours passed out of school by the unhappy victims of routine, in writing their themes and versions, leave little leisure for reading; while, on the other hand, the correction of tasks consumes time in class which would be better employed in studying the great writers of Athens and Rome. With an excess of zeal, the master often corrects the tasks of his pupils at home, consuming time which might better be given to his own improvement.

In the intervals of the lessons, the pupils of the lyceums read what the professor can hear them translate in class—an insufficient amount of practice for acquiring the art of reading in the school-period. They translate scarcely thirty lines a day in class, two or three times a week, making a small volume in a year, when the complete acquisition of this art would require the reading of more than fifty volumes. The remedy for this evil would be an initiative, on the part of the pupils, which would lead them to read outside of the lessons of the master; but, unhappily, this initiative is not encouraged. Grammar, themes, memorized lessons, and translations with the dictionary, are too discouraging, and do not dispose to voluntary effort.

One of the worst evils of the university system is, that not a step can be taken without a master. In place of exercising the pupils in the imitation of good models, which would in part dispense with his aid, they are pushed in a false direction, where they seek their way painfully, and cannot advance without help; while the professor discourages them by corrections which are renewed without ceasing.

Self-guidance is the first condition of a reasonable, improvable being. Children should learn at school how to study alone—to discover for themselves what they wish to know. In giving them no initiation, in denying them their free-will, we prepare them to resign themselves to the passive part imposed upon the nation by governments that take the initiative in all measures of social interest. We thus form subjects for a tyrant, not citizens of a republic.

III. Oral Translation.—In the beginning, translation is only a means, yet, strange to say, it is the means only that is regarded in the lyceum, without ever thinking of the end to which it conduces. And,

still worse, it is translation word by word. This occupation of the mind with words is bad in many respects: it does not appeal to the judgment of the pupil, who, in ignorance of the subject, translates them at a venture; it does not permit him directly to associate the idea with the word of a foreign language; it hinders the understanding of the text; for, the words sought by the aid of the dictionary and in the order of the foreign text being found by him in a disorder to which he is not accustomed, they do not present a clear and definite meaning. On the other hand, no two languages ever correspond word for word. In each there are a great number of phrases with no equivalents in the other, and, consequently, ideas that cannot be rendered into it. Hence it is impossible always to translate faithfully.

Translation with a dictionary, which substitutes the fingers for the intelligence, and, scorning reason, proceeds from the sign to the idea, rests on the false principle of the identity of signification in the corresponding words of two languages. Moreover, by its slowness, by the multiplicity of its interpretations, and the tediousness inseparable from its use, it repels beginners and retards their progress. Besides, to a child little versed in his own language, words translated one by one present but a vague meaning, or none at all. The text, which alone can determine it, he does not understand. Explain the unknown by the unknown; such is the vicious circle in which the dictionary places him.

It is, in part, to this illogical, repelling process that we must, in the majority of cases, attribute the failure of linguistic study. Those who say that the use of the dictionary impresses the words on the memory mistake strangely. They forget that this way of finding the meaning is not the fruit of reflection, and, consequently, leaves no traces in the mind. It is a simple acceptance of another's word, with the further uncertainty arising from the diverse interpretations of each word. A few years after leaving the lyceum, what do we know of the Latin and Greek learned with the dictionary? With this pretended auxiliary, observation and judgment are entirely inactive. The student does not choose between different interpretations, for, not knowing the thought of the author, he cannot know what would render it most faithfully.

Indirect reading or oral translation is insufficient, at all stages of advancement, to give a neat and precise idea of the thought of the writer, or appreciation of the literary value of a work. Still less, by its means, could the scholar study science with profit. The search for expressions corresponding to those of the original prevents the mind from following the logical connection of ideas, and from abandoning itself to the meditation which such serious subjects require. It is only in direct reading that the attention is left free from foreign considerations, and can enter fully into the thought of the author.

All the qualities and graces of style, which are the principal merit

of works of imagination, are lost when the attention is absorbed in the choice of words which will best render the thought of the author. Poetry, especially, cannot be read by the translator. All its beauties and merits disappear in passing into the prose of another language. Besides, the study of poetry is of no use in acquiring the materials of discourse for the exchange of thought with foreigners, or in following the progress of civilization among other people. The student should enter upon such reading only at an advanced period of study, when he can mark the rhythm and the cadence. Milton, for instance, who is on the programmes of the university, is not understood by the majority of the English, and yet our young people, who have not read more than four or five volumes of English prose, are expected to understand it in the translation!

IV. PRONUNCIATION.—The premature exercises in pronunciation, made necessary by the priority given to the art of speaking, are contrary to reason in proceeding from letters to sounds; since it is necessary to know the pronunciation in order to establish the value of the signs which represent it. It is this inversion of logical order which has given birth to reading aloud, to all the systems of written pronunciation, to all those dissertations on the letters of the alphabet in the beginning of most grammars.

The real use of reading aloud is to test the progress made in pronunciation; and, when once it is acquired, to keep it in memory by practice. But, at the beginning of study, it is a process doubly irrational: it implies that the sign leads to the thing signified previously unknown; and it presents characters to the eye instead of sounds to the ear, thus moulding the pronunciation upon the orthography, which often represents it only imperfectly; especially in the case of the English language. The attention of the pupil is occupied with the pronunciation of words without regard to their meaning, to which pronunciation is subordinate. This proceeding has become very general only because, demanding no knowledge on the part of the master, it is suited to the capacity of those who wish to teach.

Reason requires that we adapt means to ends, but reading aloud is precisely the reverse of that which occurs in conversation. In reading, we pass from the word to the idea; the orthography suggests the sound. In speaking, on the contrary, we pass from the idea to the word; the sound suggests the orthography. Reading aloud can be only a source of error to a beginner. The corrections required will never form good habits, for these are the result of the repetition of correct impressions, such as are produced by the words of the master when reading to his pupils. To speak and pronounce a foreign language correctly, we must hear it spoken habitually. The alphabetic combinations by which we represent the foreign pronunciation are equally irrational. They can only bring to the mind of the student the sounds of his own language. It is by hearing sounds, and not by

seeing letters, that pronunciation is made familiar, and yet exercises for the eye have the first place in our methods. It is impossible to represent unknown sounds to the eye by any combination of letters whatever, still less the diverse shades of intonation which characterize the speech of a people.

It is an error to believe, as is commonly done, that we cannot read a foreign text, in the sense we attach to this word, without pronouncing it, at least mentally. In the mother-tongue, the meaning of written words is conveyed to the mind only by the sounds that they represent, the ideas being *a priori* associated with the sounds. But the words of a foreign language do not recall to the student, any more than to a deaf-mute in his own language, any sound associated with the sense. There is, then, no necessity, as there is no possibility, of pronouncing it. It is, in fact, with the written signs of a foreign language, as with all other signs—we may know their value without attaching to them a sound; the Chinese characters, for example, are understood detached from all pronunciation. The young child associates the sense with the sound of words, and has no need to think of their orthography; in the same way, the student of a foreign language should associate the sense with the orthography of words, not with their pronunciation. If, as the rational method prescribes, we always pronounce the French when following with the eye the foreign text, we protect ourselves from a false pronunciation; for it will be impossible to pronounce English at the same moment when the organs of speech are occupied in pronouncing French.

V. Lessons in Memory.—Of all the exercises which most favor ignorance in teachers who are not duly prepared, and which inspire most *ennui* in students, the worst are those mnemonic exercises in which the master acts a purely passive part, and the pupil an automatic one. It is said that by such means we develop the memory of children, but for this no special effort is needed, as the culture of memory, like that of attention, is secured by the activity of the other faculties. It is more particularly in exercising the judgment that we enrich the memory with useful things. The knowledge we gather in the first years of life we owe to observation and experience—the best of masters—and it is more profoundly engraved upon the memory than all the memorized lessons of college. The mother-tongue is acquired without learning any thing by heart.

Those who, in teaching their pupils to speak a foreign language, give them words to learn, to form into phrases, commit a triple error. In the first place, the child does not learn to talk by passing from words to phrases. In the second place, in order to speak, he learns to understand what is said to him. In the third place, no mother ever attempted such a proceeding: the instinct of imitation alone suffices the child in learning to speak.

The expression of thought is not aided by learning extracts from

authors, because, for the most part, these extracts contain not a phrase or an idea that would aid in conversation. In this work, the attention is directed exclusively to words, and the memory is aided by their juxtaposition. By means of repetition they are revived in the mind in their order of succession, each word suggesting that which follows. The more we repeat the lesson in order to retain it, the more easy and rapid the recitation, the more the text escapes analysis and the will. Excellent as the exercise may be in pronunciation and oratory, it is inefficacious as a means of learning to speak. To learn a model by heart, no more teaches to speak, than tracing a drawing-model teaches to draw.

The monotonous repetition of a text is a mental operation diametrically opposite to that employed in the expression of thought. To speak is an act of judgment, to recite is an act of memory: the first is spontaneous, the second mechanical; by this we associate words with ideas, by that we associate words with each other; in the one we are masters of an ever-changing phraseology, in the other we are the slaves of an invariable text. In speaking, the mind is exclusively occupied with ideas; words present themselves as consequences. In reciting, on the contrary, it is words that absorb the attention, ideas following in their suite, and sometimes even are not present to the mind; children often fail to understand what they know by heart. Montaigne said, with reason, "To know by heart is not to know."

As to dialogues or exercises in conversation, whatever the number of them with which the student has charged his memory, he can say nothing beyond some trivialities which it has pleased the compiler to group together. His individuality disappears, and he is only the servile echo of phrases that have been imposed upon him. It is manifest that the art of speaking, of managing a language at will for all the needs of conversation, consists less in remembering a great number of ready-made phrases, than in the power of constructing at will, and instantly, those that meet the necessities of the moment. It will be more profitable, then, to follow the process of Nature, which consists in constructing phrases for one's self, on a given model, by analogy.

The time given to learning dialogues profits little; for, most commonly, they are forgotten long before there is occasion to use them. Phrases learned by heart are rapidly forgotten. Not only are these lessons worthless, but they require painful labor and considerable time; they are, for young people, an incessant cause of disgust and punishments, which can only inspire aversion for the study. They do not even serve usefully to cultivate the memory; for the power to retain these words in a given order is of no use except to actors in learning their parts.

We cannot by special processes obtain the general improvement of a faculty. The power or aptitude of a faculty never transcends the limits assigned to it by the special exercise to which we submit it.

All the faculties are subject to this law. Thus, persons in whom the ear, exercised in melody, distinguishes the most delicate tones in music, are not those who best seize the pronunciation of language. The eye exercised in colors does not better appreciate form and distance; and reciprocally.

Again, the development of the intellectual faculties is always conformed to the kind of exercise which produced it. Those who have learned much by heart, learn easily by heart; but they are not in consequence better able to recall facts, dates, localities, forms of objects, subjects of discourse, the details of a profession—nothing, in fact, which is useful for the exigencies of active life. It is to falsify Nature to ask from the memory of words that which can alone be given by the memory of things.

All the time spent by a child in learning its lessons by heart is lost, as far as concerns the exercise of judgment and the practice of language. In a class, the great majority of pupils remain idle while waiting their turn of examination. As to the master, what does he do? He does not instruct. Whatever he knows, his knowledge is a dead page for his pupils. He who, in his teaching, does not go beyond the contents of the book is unworthy to be a teacher.

It is with mnemonic lessons as with other useless drudgery imposed on the young, which, without profit, puts their intelligence to torture. All these preparatory exercises end in nothing practical. They only retard the acquisition of direct reading. If the employment of these diverse processes continues in our lyceums, they will give no better results than they have given in the past, with professors probably as clever, as zealous, as anxious to do well as their successors.

VI. Conclusion.—We would not object to the length of time spent in classical study, as we have hitherto had a right to do, if the pupils, giving not more than eighteen months or two years to acquire that which alone is useful in the ancient languages, the art of reading them, derive advantage from them during the remainder of their school-life, however long it may be, in cultivating their minds, and extending their knowledge of the national idiom. Unhappily, this is not the policy in public instruction. As regards the living languages, students leave the lyceum, for the most part, without having attained any of the objects of study. They are persuaded that they have nothing to learn, when they know by heart all the rules of grammar, have written all the themes they contain, and learned a volume of dialogues; when they commence to translate fluently, to read aloud good or bad, and to make correctly grammatical and logical analyses. However, nothing of all this is really the practice of language. Nothing of all this finds its application in the commerce of life. They know the language *by rule*, which means, in most cases, that they can neither read, nor understand, nor speak, nor write it.

It is particularly in the study of classics that routine is pernicious

to the young. The university system, which involves an enormous expense of time and money, is no longer in harmony with our civilization. It disregards the diversity of talents, the specialty of individuals, and casts all minds in the same mould. It conforms neither to the laws of Nature nor the needs of modern society; it calls in play neither spontaneity, nor curiosity, nor imitation; it surcharges the memory to the prejudice of the judgment, and aims at verbal acquisition rather than mental culture.

It does not embrace any of the great classics. It reverses the order of reason in passing from words to phrases, from theory to practice, from the art of writing to the art of speaking. Finally, it sacrifices the great majority of students to a few privileged ones, and all are too much occupied in things of the past to the exclusion of that knowledge which the progress of civilization has made indispensable.

It is to be wished that the Minister of Public Instruction may, in the interest of our country and society at large, honor with his attention the preceding observations, and use his powerful influence in favor of the substitution of reason for sterile routine in our schools! To appeal to the past, as is done at the university to justify its proceedings, is to hold intelligence in tutelage, and to condemn France to immobility. The world, in growing old, adds to the experience of man. Enlightened as we are by what we have received from our fathers, we commence life in the most favorable conditions. We ought to know more than they, and be more able to distinguish truth from error. Let us go forward with our century. It is time to leave the rut of tradition.

Let the fathers of families unite in appealing to the minister not to permit our lyceums to perpetuate a system of teaching that favors the ignorance of the people as a means of government. But let him take in hand the great work of the regeneration of linguistic study. We must apply to mind, as to matter, new powers and new processes. France will awake to intellectual life, and rise to a level with the most enlightened nations, only when its university teaching is completely conformed to the laws of Nature and the demands of reason.

PARALLEL.

BY THE RATIONAL METHOD.	BY THE METHOD OF ROUTINE.
1. We follow step by step the indications of Nature.	1. We follow in nothing the precepts of Nature.
2. Curiosity and imitation are the source of progress.	2. We are not aided by these two powerful instincts.
3. We go straight to the object by example and practice.	3. We start from rules and preparatory studies.
4. Grammar becomes the consequence of language.	4. Language is made the consequence of grammar.
5. We understand the foreign text before we translate it.	5. We translate the foreign language before we understand it.
6. The art of writing is the last in the order of study.	6. The art of writing is the first in which the pupil is exercised.

7. Pronunciation is acquired without effort by the ear.	7. Pronunciation is acquired with difficulty by reading.
8. We understand foreigners as well as natives.	8. We cannot understand the foreign language when spoken.
9. Nothing is learned by heart.	9. Much time is spent in memorizing lessons.
10. All our processes tend to prevent mistakes.	10. All the processes tend to produce mistakes.
11. We have no need of a master for pronunciation.	11. We cannot learn pronunciation without the help of a master.
12. We are soon able to think in the foreign idiom.	12. We never come to think in the foreign idiom.

A SHORT STUDY OF BIRDS'-NESTS.

By CHARLES C. ABBOTT, M. D.

I.

HAVING had many opportunities of examining the nests of those birds habitually breeding throughout Central New Jersey, during the past fifteen years, and so, familiar with the construction and location of such nests, I have, since the publication of Mr. Wallace's essays on "Natural Selection," in 1870,[1] endeavored to determine if the theory there expressed was applicable to the birds that are common to the locality we have mentioned.

In so studying birds'-nests, I have carefully avoided prematurely arriving at any conclusions that might influence my judgment when subsequently examining a series of nests, and therefore I believe the notes made concerning the construction of each nest, and the inferences drawn, are exact in the former case, and justifiable in the latter.

At the very outset, I found a careful study of the courtship of birds essential to a proper appreciation of their subsequent habits, and learned, not at all to my surprise, that marriage among birds, as among mankind, is not universal, but that both bachelor and spinster birds of every (?) species constitute a fraction of the ornithic population of our woods and fields.

I reached the above conclusion in this way: Having carefully gone over a given extent of ground, and noted every nest, say of the cat-bird (*Galeoscoptes Carolinensis*), I have then endeavored to learn about or precisely the number of individuals of this species frequenting the same extent of territory. As birds, during the breeding-season, do not wander any very great distance from their nests on the one hand, nor from the locality whereat they halt on their arrival in early spring, on the other hand, it is not very difficult to reach a very

[1] Essays on "Natural Selection," by A. R. Wallace. Macmillan & Co., London and New York, 1870 (pp. 211–263 inclusive).

close estimate of the numbers of each species occupying a locality, any given season. Thus, during May and June, 1873, I found eleven nests of the cat-bird in a given area, and feel confident that I recognized twenty-seven individuals of this species. If this is correct, then there were five cat-birds not nesting, and, I should judge, all male birds. It will be remarked that I overlooked the nests of these "extra" birds. This I believe is impossible. By going over a given space—an acre at a time—prying into every nook and cranny, climbing every tall tree and searching over every small one, as well as bushes and brier-patches, it is hardly possible to overlook any nest, especially so large and conspicuous a one as that of the cat-bird. The habits, too, of non-nesting birds differ from those then breeding. They are much less restless, do not chirp and twitter, or exhibit distress when closely followed, as in the case of nesting-birds.

Having carefully examined a bird's-nest which seemed to agree most nearly with the published descriptions of such nests, I then noted each nest found and marked the amount of variation in the construction and position. Take, for instance, the nest of our very common robin (*Turdus migratorius*). Here we have a nest largely constructed of coarse twigs and grass, lined with "a cup-shaped fabric of clay or mud," this mud, again, being covered with finer grass, horse-hair, and occasionally a few feathers. This nest is an excellent one to study for degrees of variation in construction; and here note these differences. During the past spring and early summer, we found thirty-two nests of the robin in an area of about four hundred acres.

Of these thirty-two nests I will speak, principally, as to their construction, especially with reference to the care exhibited in the *mud-lining*, and refer but incidentally to the positions of the nests.

Eleven nests were what might be called "typical," following the description given by Dr. Brewer in the latest work on North American ornithology.[1] In the eleven nests the mud-lining was complete, extending to within about an inch and a half of the rim, or top of the nest. In fourteen, the mud-lining was more or less incomplete, although always extending over the bottom of the nest, i. e., so much of the interior surface as the eggs or very young birds rested upon. Without an exception, I believe, the fine grass and hair lining the interior of each nest were in greater amount in proportion as the mud-lining was imperfect; so that, in some instances, the mud being concealed, the nests were very similar to those of other thrushes. The remaining seven nests were altogether "abnormal," and, noticeably, each of these seven nests was in such position as a robin would not be supposed to select. A careful study of the surroundings, however, showed that there was always some outside advantage, such as

[1] "A History of North American Birds," by Messrs. Baird, Brewer, and Ridgway. Vol. i., p. 27. Boston, 1874.

proximity to abundant food, and this may have had some influence in the choice of location. As an instance, one of these seven nests was placed in a deep cleft in the trunk of an apple-tree. It had a southern exposure, was protected from rain by the trunk and branches of the tree, and, altogether was admirably located. But, as the tree itself had an abundance of branches, and for many summers had had nests upon it, there seemed to be some reason in the location now first occupied. What, indeed, was the cause of this change from the branches to the cleft, I could not discover. The nest itself was merely a few coarse twigs for extra support of the "clay fabric," which was placed so as to resemble a modified cliff-swallow's nest more than that of any other bird. If, now, young birds build nests through imitation, then the young robins reared in this nest will seek out somewhat similar situations for their own nests; but if such a locality did not suit the bird's mate, then a nest in a more exposed position would be built, but, I doubt not, with some of the peculiarities of the nest in which it was reared.

In comparing the eleven typical nests of the robin, it could not but be noticed that minor differences or peculiarities existed. These small variations were such as size, which was, in fact, considerable; in shape, some of the nests being rather oval than circular; in the choice of material for the interior lining, which, I am sorry to say was, in one instance, suspiciously similar to the lining of the nest of the chipping sparrow, and was probably stolen. Indeed, among robins, as well as all other birds, there are individual rogues, as well as cross-grained, scolding wives and husbands.

Taking a careful survey of the whole thirty-two nests, they suggested at once an ordinary village: there were handsome structures, such as opulence builds, and very modest ones, such as those in straitened circumstances are compelled to occupy; and, while the same causes for this variation in dwelling-places does not obtain among birds as among mankind, causes do exist among the birds, in many ways analogous. For instance, there are energetic birds and lazy ones. There are plucky birds that will overcome obstacles, and despondent ones that are easily cast down; and will not this of itself account for a great deal in the variations of birds'-nests? Can it be doubted that birds differ greatly in their temperaments? Who, that has kept canaries, has not noticed that, while some are cross, others are affectionate, others lively, and, again, others moody—that their dispositions were nearly as varied as in mankind? If it is admitted that variation in disposition exists among birds, may we not go a step farther, and claim also differences in mental ability—that, in plain language, the "smarter" bird will build the better nest? One reason why nests do not vary more than they do, simply, is—a mud-lined nest being *best* suited to a robin's welfare—that a bird reared in a poorly-constructed nest may be of greater ability and more

energetic than its parents, and this, joined with the fact that the bird's mate may have been reared in a nest of perfect construction, of itself would tend to remedy, in part, the defects its partner might allow; these facts together would certainly secure an approach to, if not the complete attainment of, a "typical" robin's nest. So, as the years roll by, the nest of the robin would remain substantially the same, while the amount of variation that now exists would be perpetuated, and probably very slowly increased.

Why indeed, a robin should line its nest with mud, and a cat-bird should not, is probably past finding out, but, as changes gradually brought about by man's agency have already effected changes in the habits of some of our birds, so these same changes, ever in progress in the haunts of our robin, may cause these birds to gradually omit this lining of mud in their nests, and so make them more like the nests of other thrushes; just as the cliff-swallow, with us, no longer places a "bottle-neck" opening to its mud-built nests. There is an instability in the whole range of the habits of birds, going hand-in-hand with the undoubted tendency to variation in their anatomical details. Natural selection, or whatever may be the governing impulse that controls it, also indirectly causes the range of variation in the details of the construction of their nests, inasmuch as these variations of habit are the necessary result of changes wrought in the physical construction of the creatures themselves, for, stripped of the haze that metaphysics has gathered about it, as a bewildering gloom, we can see in the operations of the mind, in man and bird, only the curious results of the workings of those fatty atoms, intimately combined, we call the brain; and no argumentation can separate this brain and mind. They are just as interdependent, and parts of a single whole, as the eye and sight, the nose and smell, hearing and the ear, the circulation of the blood and the beating of our hearts.

II.

A nest of totally different character, that of the well-known Baltimore oriole (*Icterus Baltimore*), was more carefully studied by the writer, inasmuch as it afforded more marked variations from what may be considered a "typical" form of the structure.

Mr. Wallace has shown that, where a nest is so constructed as to conceal the sitting bird, in all such cases, the birds are of bright, showy plumage, and would be easily detected by birds of prey, if not concealed when occupying their nests. Of the family *Icteridæ*, to which our Baltimore oriole belongs, Mr. Wallace says, "The red or yellow-and-black plumage of most of these birds is very conspicuous, and is exactly alike in both sexes. (*This is not true of the Baltimore oriole, the female of which is much less brightly colored.*) They are celebrated for their fine, purse-shaped, pensile nests." There are now two considerations worthy of attention, with reference to this bird

and the character of its nest. In the first place, as the male bird is much brighter in the color of its plumage, would it not require a concealing nest if it assisted in incubation? Now, does the male bird assist in covering the eggs? It unquestionably does.

Secondly, if the bird-concealing nest, a "pendulous and nearly cylindrical pouch," as described by Dr. Brewer, is constructed solely with reference to the protection of the parent-birds, would it not be within the range of probabilities that, the danger no longer existing, the labor of constructing so elaborate a nest would be abandoned. Has this actually occurred? During the summer of 1872, I found nine nests of the Baltimore oriole within a comparatively small area; in 1873, I succeeded in finding seventeen nests in an area nearly ten times in extent; and during the present summer (1874) I found thirteen nests in an area of the same extent as that examined in 1873. These thirty-nine nests I classified as follows: Of the nine nests of 1872 that I examined, six were so constructed as to effectually conceal the sitting bird, and three were sufficiently open at the top to give a hawk, hovering above it, a view of the bird.

Of the seventeen nests of the oriole which I found and inspected during the summer of 1873, eleven of them were "bird-concealing" in their shape, and the remaining six like the three I found in 1872, i. e., open at top.

During the present summer, Baltimore orioles have been unusually abundant, and, of the thirteen nests I found, eight were open at the top, and five were long, pendulous pouches, that wholly hid from view the sitting-bird.

Bearing in mind the supposed reason for building a nest that would conceal the parent birds when occupying it, I noted down the exact location of each of these thirty-nine nests. In every instance those nests that concealed the sitting bird were at a considerable distance from any house, in uncultivated parts, the larger number on an unfrequented island, the others on elm-trees growing on the banks of a lonely creek. In both of these localities, sparrow-hawks (*Tinnunculus sparverius*) were frequently seen—they are nowhere so numerous as some seventy years ago—as compared with the neighborhoods selected for the building of the open-topped nests, all of which were in willow and elm trees in the yards of farm-houses, and in full view of the people continually passing to and fro beneath them. The conclusion drawn from the study of these nests was, that the orioles, knowing there was much less (if not total) absence of danger from hawks, therefore constructed a less elaborate nest—one which answers every purpose of incubation, and yet does not conceal them when occupying it.

Of the nests that did conceal the sitting bird, every one was really open at the top, and the bird entered from above. The weight of the bird, when in the nest, appeared to draw the edges of the rim together sufficiently to shut out all view of the occupant. The rims of

these nests that, when occupied, concealed the birds, were all much smaller and the nest itself deeper than in those nests where concealment was not considered in the construction, these latter being in every way much like the nest of the orchard oriole (*Icterus spurius*).

Originally, in all probability, when its enemies were more numerous, especially the smaller hawks, the nest of the Baltimore oriole was perfectly closed at the top, and with a side opening; but, of the many scores of this nest that we have met, we have never seen a nest of this bird so constructed.

The very fact of the Baltimore oriole constituting a partial exception to Mr. Wallace's supposed law of birds'-nests is, we think, here shown to be a proof of the correctness of his theory.

SKETCH OF THE LIFE OF FRANCIS HUBER.

By Mrs. S. B. HERRICK.

FRANCIS HUBER was born in Geneva, July 2, 1750. His father, John Huber, was a man of many and varied gifts; he was considered one of the wits of the day, and was an accomplished musician, poet, painter, and sculptor. The art of cutting landscapes and silhouettes from paper may almost be called his creation. He attained such proficiency in executing likenesses in this way that, with his hands behind him, he tore from a card a correct profile of Voltaire. The curious combination of talent and caprice which characterized most of his work is well illustrated by one of his attempts: he executed a profile of Voltaire, upon one occasion, by allowing his cat to bite from a slice of cheese portions which he successively presented to her! Besides his lighter social and artistic gifts, he possessed keen powers of observation as a naturalist and considerable facility with the pen, as is shown in his "Observations sur le Vol des Oiseaux de Proie," Geneva, 1774.

John Huber transmitted to his son most of his tastes, without that discursiveness and caprice which so fatally marred his own career. The boy, from his early childhood, attended lectures at the Genevan College. The library, the cabinet, and the observations of his father, early roused in him an ardent love for natural science; he had begun intelligently to observe Nature at an age when other children seem hardly aware of her existence. Before he was fifteen years old, he had completed a course of physics under De Saussure, and familiarized himself with chemical manipulation in the laboratory of a relation, who was an obstinate alchemist.

Intense application, together with the habit of reading late at night, by dim lamp-light, or even by the light of the moon, seriously impaired

his health. At the age of fifteen he found himself not only utterly prostrated in strength, but threatened with blindness. His father, in alarm, took him to Paris, to consult the celebrated Tronchin, who ordered him to the country. At the village of Stani, near Paris, he led the life of an ordinary peasant-lad, following the plough, and occupying himself in other farm-work. This regimen proved efficacious, and he returned to the city in full and vigorous health.

The oculist Wenzel, after an examination of his eyes, pronounced the disease incurable. One eye was affected with the *gutta serena*, or amaurosis, the same disease which caused the blindness of the poet Milton, of which he says:

"So thick a *drop serene* hath quenched their orbs."

The blindness of the other was caused by cataract, a disease, in our day, often successfully treated. The science of the oculist was, then, far from the perfection which it has since reached, and the operation was considered too hazardous to be attempted.

Happily, before the darkness closed down upon him, he had seen and loved Marie-Aimée Lullin, daughter of one of the syndics of the Swiss Republic. The childish love which had sprung up, at a dancing-school, between the boy and girl, grew with years into a deep and life-long devotion. In spite of the bitter opposition of her father, which amounted to persecution, Mdlle. Lullin refused to give up her lover; but Huber was filled with fears lest his growing infirmity should alienate her. Under the influence of this dread he would hardly acknowledge to himself the advance of the disease. As long as he could at all perceive the light, he spoke and acted as if he saw. The habit of expression thus formed left its impress upon his future style. One cannot read the graphic descriptions of his experiments, as given by himself, and fail to notice the frequent recurrence of such expressions as "*I have often seen*"—"*I had the satisfaction of seeing*"—or even, at times—"*I saw with my own eyes.*" There is a profound pathos in this apparent obliviousness to his affliction, when we thus trace it to its source.

These fears, however, proved groundless, for Mdlle. Lullin, as soon as she reached her majority, married him. During the forty years of their married life, her tenderness and devotion toward her husband were unfailing. She was his reader, his secretary, his observer; he said of her, in his old age: "As long as she lived I was not sensible of the misfortune of being blind."

Besides his wife, Huber was eminently fortunate in his assistant, Francis Burneus. This man, who had entered the family in the capacity of a servant, his master soon discovered to be "born with the talents of an observer. . . . It is impossible," Huber again says, "to form a just idea of the patience and skill with which Burneus has carried out the experiments which I am about to describe . . . . he counted pain and fatigue nothing compared with the great desire he felt to

know the results. If, then, there be any merit in our discoveries, I must share the honor with him; and I have great satisfaction in rendering him this act of public justice."

Huber practised Burneus in the art of observation; comparing his results with those of other investigators. He directed him by a thousand questions, adroitly combined, till fully satisfied of his fidelity and accuracy. At the first issue of the book, which was the record of their joint labor, the naturalists of Europe looked askance at the marvelous revelations of bee-economy made by a blind man aided by a peasant; but, as knowledge upon the subject grew, prejudices melted away, and there is scarcely a fact recorded by Huber which subsequent investigation has not again and again confirmed.

The marvelous activity of mind, which to many men would have proved only a torment, was to Huber a source of the deepest delight. His love of music, and proficiency in the art, beguiled many hours, and added greatly to his social enjoyments. He had made himself master of counterpoint, and was able, from the dictation of the bass of a musical composition, to arrange the harmonies. The whole piece or song would be dictated to him, in this way, phrase by phrase, and a single repetition was all-sufficient.

He invented, for his own use, a printing-machine, by means of which he could correspond with his absent friends; and he was enabled to indulge his fondness for walking in the opon air by means of another contrivance of his own. He caused knotted cords to be stretched along the borders of all the rural paths around his house; by means of the cord he could guide himself, and the knots informed him what point he had reached.

Soothed by every appliance which ingenuity and ample means could afford, surrounded by the tenderest affection and the keenest sympathy in his pursuits, his darkened life was full of sweet compensations. But it is in himself, rather than in the circumstances of his life, that we find the sources of his tranquil happiness. He retained to extreme age the tenderest affection for his friends; he showed to the last the untouched freshness of delight in Nature, the boyish candor and directness, the noble enthusiasm, the quick sympathy with youth and its interests, which so generally characterize men of science, and which offer one of the most unanswerable arguments in favor of the ennobling influence of such pursuits.

"When any one spoke to him on subjects which interested his head or heart," says De Candolle, "his noble figure became strikingly animated, and the vivacity of his countenance seemed, by a mysterious magic, to animate even his eyes, which had been so long condemned to darkness. The sound of his voice had always something of the solemn. 'I now understand,' said a man of wit to me one day, who had just seen him for the first time—'I understand how young people willingly grant to the blind the reputation of supernatural inspiration.'"

Huber retained his faculties to the last. He wrote to one of his friends on the 20th of December, 1832, and two days later he sank to rest, without a pang, in the arms of his daughter, Madame de Molin, in the eighty-second year of his age.

The work, in his own department, which he has left behind him, is marvelous in its accuracy and its fullness. Facts which had eluded observers from the days of Aristomachus of Soli down to those of Bonnet, yielded to the patience, tact, and ingenuity of Huber. It is hard to decide which is most admirable in him—his life-long devotion to one purpose; the patience and caution with which he questioned and cross-questioned Nature by experiment; or the lucidity and picturesqueness of his descriptions of his work. The latter quality, it is probable, was the direct result of his deprivation. It was necessary for him, out of the disjointed answers and remarks of his observers, to form a perfect, rounded mental conception of the facts in themselves and in their relations. This perfect comprehension in great measure insures a luminous style; obscurity of style being much more frequently a result of confusion of ideas than of a mere awkwardness in the use of words.

Huber's work was first recorded in the form of a series of letters addressed to M. Bonnet, and called "Nouvelles Observations sur les Abeilles," 1792. Afterward, in the later editions, several papers on the "Origin of Wax," the "Sphinx Atropos," "Bee Architecture," and other topics, were incorporated into the same volume. Many of his experiments and observations were made at the suggestion of Bonnet, and it was upon his recommendation that Huber constructed his "single-leaf" and "book"-observing hives. The first of these was made to contain a single comb, but, fearing that the bees, who are taught by Nature to build several parallel combs, might manifest change of habit or modification of instinct under new conditions, he also caused to be made another hive, by which he could correct the observations made upon the first. This second hive was so arranged that each frame could be turned back, like a door, upon hinges.

The first observations which Huber records are those upon the fertilization of the queen. Many theories had been advanced upon this subject, by Swammerdam, Debraw, Hattorf, and others, supported by experiments which, to most minds, would have seemed conclusive; but Huber was not satisfied till he again and again repeated, with every precaution and under every condition, the experiments made by his predecessors. These experiments, made by himself and others, he describes with his usual clearness, and from them he deduces the following singular facts, which have been a thousand times confirmed:

The queen-bee, which is the only perfect female in the hive, is fecundated on the wing, and this one fecundation suffices to fertilize the hundreds of thousands of worker-eggs which she lays during her life, of from three to five years. If the impregnation of the queen be de-

ferred until the twenty-first day of her life, she not only lays nothing but drone-eggs, but her instincts are also impaired. Perfect, fertile queens always discriminate when laying; they deposit the drone and worker-eggs in their own peculiar cells, but the queens whose impregnation is retarded are indifferent, and lay their eggs anywhere. Previous to Huber's time, it was believed that the worker-bees, like worker-ants, carry away eggs thus misplaced, and deposit them in the proper cells; but he ascertained the fact that they carry away such eggs only to devour them. By some unknown means the bees can distinguish a worker from a drone egg; and, if, by an oversight, a worker-egg is allowed to develop into a larva in a drone-cell, or *vice versa*, the bees cap over the cell with the flat or convex cover peculiar to its inmate, without reference to the size of the cell upon which they are working.

Réaumur, and most other naturalists, had believed that the queens deposit, in royal cells, a peculiar egg, which develops into a queen; but Schirach, a German clergyman, announced, toward the close of the last century, a discovery which created not a little interest. Some doubt still hung over this discovery, which was entirely dispelled by Huber's observations. Schirach stated that the bees can, by peculiar treatment, rear a queen from a worker-brood.

If a swarm of bees find itself suddenly queenless, the workers immediately select the larva of a common bee, not over three days old; they enlarge this cell by cutting down the partition-walls between it and two adjoining cells, destroying their inmates, and then they supply the remaining worm with food, differing in quality and quantity from that of the workers. The nursery of the royal heir is elongated, and finally capped over with a peculiar covering. In sixteen days after its exclusion from the egg this larva becomes a queen. From increase of space, accession of heat, and the different quality and quantity of food given to the worm, that which would have become a worker becomes a queen. By this change of treatment, its anatomy and physiology, its instincts and functions, the time necessary for its development, and the length of its life, are all utterly changed. The queen performs but one office in the hive, that of supplying her realm with subjects, while the workers perform all the multitudinous offices which the economy of the hive demands. The whole structure of the queen and workers is coördinated to their functions; she possesses the ovaries, which in the worker lie folded away in a germinal form, while they possess all the organs needed for their peculiar work—strong mandibles, powerful wings, pollen-basket, pincers, brush, wax-pockets, and honey-receptacle. Though queens are often made from worker-larvæ, Huber observed the old queen laying in royal cells, when the hive is about to throw off new swarms, and more than one queen will be required.

He also determined that some few workers, which have partaken

of the royal jelly, have their reproductive organs partially developed, and lay drone-eggs. This fact was reached by an experiment which required great courage and an almost incredible patience. Burneus, of his own free will, caught, held, and carefully examined every bee of two swarms in which fertile workers were suspected. This required *eleven days* of steady labor. "During all that time," says Huber, "he scarcely allowed himself any relaxation except what the relief of his eyes required." Each bee, after examination, was transferred to a glass hive, which was watched; finally, a bee was caught in the act of laying, and was dissected: small ovaries were detected containing a few eggs, but in all other respects it was a perfect worker.

Huber also discovered the bitter animosity of the queens toward each other. He observed the first-hatched queen, as she emerges from her cell, traverse the comb till she finds a royal cell, which she tears open, in apparent fury, and then stings the helpless pupæ to death. This she repeats again and again till she has destroyed every possible rival. If, however, two queens emerge simultaneously, the bees clear a space, and stand back and watch the conflict, which must end fatally to one or the other. The two queens attack each other, but, if, during the fight, they happen to find themselves in such a position that, by closing, each would kill the other, they withdraw, and begin the combat afresh. As soon as either secures such an advantage of position that she can sting without being stung, the fatal thrust is given.

Réaumur made the suggestion that it is the queens themselves who rid themselves of their rivals; but he had been strongly opposed by the German naturalists, who contended that the worker-bees disposed of the interlopers. What Réaumur advanced as a conjecture Huber founded upon the impregnable basis of fact.

If a strange queen be introduced into a swarm possessing one of their own, the workers generally surround her, and quietly detain her prisoner till she perishes of hunger, but do her no direct injury. The only instance where workers were ever known to sting a queen was once, during a general *mêlée*, and then it seemed pure accident. If, however, the stranger queen, by accident, passes the sentinels at the door, without being challenged, or is introduced by the experimenter into the hive, for the purpose of observation upon the point, and brought into close quarters with their own queen, they insist upon a battle, and restrain the motions of either if they seem inclined to fly.

After the loss of a queen, by a swarm, if a strange queen be introduced, her reception will depend entirely upon the time which has elapsed since their bereavement. At first, they refuse to be comforted, and reject contumeliously any attempt to replace their loss. After eighteen hours, they begin to consider the matter, and, in twenty-four, receive, with royal honors, any queen offered to them.

The yearly massacre of the drones, though a well-known fact, Hu-

ber was the first to observe in its details. It was seen through a glass table, upon which a hive, deprived of its bottom-board, was placed. He also ascertained that the massacre never takes place unless the swarm possesses a fertile queen, and the swarming-season is over.

By aid of the microscope, Huber proved that the queen-bee is truly oviparous—that her eggs are true eggs. He saw the worm grow to maturity within the transparent walls of the egg, rend the pellicle, and emerge. The idea that workers brood the eggs he also dispelled, by repeated observation of the fact that the eggs hatch equally well when removed from the care of the bees, and that the workers frequently enter empty cells and remain quietly there, evidently taking repose.

His observations upon the spinning of the cocoon were made through the walls of blown-glass cells, into which the egg was removed. The drones and common bees spin complete cocoons; the royal larva, on the contrary, spins an imperfect one, enveloping the head and thorax, but reaching only to the second ring of the abdomen. This is evidently the result, not of any peculiar instinct, but of the conformation of the cell, for the royal cocoon is complete if it be spun in a common cell. In ordinary cases, if the royal cocoon were complete, it would be impossible for a queen to destroy her rivals in the state of pupæ.

By repeated experiment, he showed that the size of a cell has no modifying effect upon the development of a bee, except by retarding its growth, if it be too small: a common bee is the same size, whether reared in drone or worker cells. In determining this point, many interesting facts in regard to the instinct of queens and workers were ascertained. A fertile queen refused to lay worker-eggs in drone-cells, though evidently oppressed with them: when, however, he introduced worker-cells, artificially supplied with a drone-brood, the bees emptied the cells, and the queen laid in them, *five or six* eggs in each. With his usual judicial fairness, Huber remarks upon this inconsistency in the instinct of queens. They refuse to lay drone-eggs in worker-cells, and yet here is a queen which deposits five or six eggs in a single cell; the drone-egg in the worker-cell would produce a small though perfect drone; the five or six worker-eggs in the same cell, if they all remained there, would produce nothing.

Huber concludes, from a number of experiments made in this direction, that, though the queen knows what kind of egg she is about to lay, and so deposits it always in the proper cell, yet she does not *determine the sex* of the egg, as is believed by many of the most distinguished modern apiarians. He also found that it is impossible to compel bees to rear a worker in a common cell, if it has been supplied with royal jelly. If the colony be queenless, they enlarge the cell into a royal cell; and, if they already possess a sovereign, they destroy the worm and devour the royal jelly.

When the first great drone-laying begins, the bees construct a number of royal cells, sometimes as many as twenty-seven. In these the queen deposits eggs on successive days, so that, when she leads off the new swarm, another queen may be ready to take her place; and also, that if the swarm be vigorous enough to throw off several colonies, each may be provided with a leader. During this season the ordinary instincts of the workers seem reversed; they hinder the queen, if she seems so disposed, from destroying the royal pupæ contained in the cells.

A common bee, when it reaches maturity, makes its way, without help, out of the cell, and it is for some time too weak to fly. A queen, however, is guarded by the bees; she is closely watched, and constantly fed through a small aperture in the covering of her cell, till she has attained sufficient strength to fly. The presence of a developed and imprisoned queen is generally made patent by a peculiar note which she utters, called piping. Above the busy hum of the hive this sound may be distinguished; it seems to be the expression of her impatience at her imprisonment, and is the usual precursor of swarming.

Another note, peculiar to the queen, Huber mentions. This he calls the *vox regalis*, and he states that its utterance invariably struck the bees motionless. It has not been observed by modern apiarians, and yet the best among them do not deny the fact, because of his usual exactness and caution.

Huber describes the process of swarming in minute detail. Toward the close of the drone-laying season, when numbers of the drones, and some of the queens, have nearly attained maturity, he observed the old queen rapidly passing over the combs. She created an agitation wherever she went, which did not subside after her departure, but communicated itself to all the bees in the vicinity. Finally, the whole swarm appeared to be in a violent state of excitement, and large numbers issued from the hive with the queen at their head. During the agitation, which precedes swarming, the thermometer rises from between 90° and 97° to 104°. "This heat is intolerable to bees," says Huber; "when exposed to it, they rush impetuously to the outlets of the hive, and depart." Swarming is occasioned by excessive heat, quite as much as by an overstocked hive. The initial cause of the queen's agitation is not known, but it always communicates itself to the whole swarm, whatever its cause may be.

Queens raised from the larvæ of workers had been called mute, because the piping had not been observed in them; but Huber discovered that it was only because they are not detained in captivity. He held one in confinement, and found her piping quite as vehement as that of her sisters, reared from the beginning in royal quarters.

The instinct of worker-bees, which is usually so unerring, sometimes fails them in the most unaccountable way. Though they detect drone-eggs in worker-cells, and worker-eggs in drone-cells, they seem

to be ignorant of the mistake made when a drone-laying queen deposits her egg in a royal cell, and dose the poor fellow to death, trying to make a queen of him. In his investigation upon workers, Huber discovered that they were of two kinds, wax-workers and nurse-bees: the former are larger and stronger, and possess more perfect organs for the secretion of wax than the latter, though no other difference can be detected, and this seems merely the result of being reared in smaller cells.

Swammerdam states that a blind or mutilated queen ceases to lay, and that the workers of her hive no longer continue their labors, or make any collections, as if aware that it is useless to do so; but Huber shows that this is not wholly true. One queen deprived of her four wings, and another of one antenna, continued to lay and to receive the homage of her subjects as before: but the amputation of both antennæ produced very different results. Queens, drones, and workers, when deprived of both antennæ, lose their instincts, and seem to be subject to a kind of delirium. They wander aimlessly about, are unable to direct the proboscis for the reception of food, seek the door of the hive, and soon perish. A fertile queen, besides these symptoms, which are common to all under the deprivation, drops her eggs about the comb, and manifests no recognition of, or hostility to, another mutilated queen.

The first part of Huber's work here concludes with some valuable suggestions in practical apiculture. The observations in the second part were made by the aid, first, of his wife, and afterward by his son, Pierre Huber, so well known through his investigations upon ants.

In the year 1809, a bee made its appearance before one of Huber's hives, which seemed to differ from its rightful inhabitants only by a darker and less downy coat. Violent contests took place every day between the native and the alien bees. Aristotle mentions "a black bee which is called a thief," but Huber seems never to have heard this suggestion, and supposed them to differ from his own bees in some physical particulars. He, therefore, submitted them to Mdlle. Jurine for dissection, and she discovered not only in them, but in all workers, ovaries—a fact which entirely overturned the theory of neuters which had held its ground for so many centuries. The worker is not sexless, but is an imperfectly-developed female. This discovery throws some light upon the wonderful change made by physical conditions in the development of the worker-larvæ. With ordinary cell and food, the worm develops into an insect possessed of feminine instincts, in the provision which she makes for the young, and for the future needs of the swarm; while, with the larger cell and different food, the same larva would develop into a physically perfect female, which, at the same time, is wanting in all maternal instincts—a divergence which is undoubtedly one of the most marvelous in Nature, but a divergence only, not a contradiction!

The senses of bees were the next subject of investigation, and we will give, in brief, the results which Huber reached. The lenses of the bee's eyes are not adjustable; and, though they can see accurately to great distances, they seem blind to objects close by. Bees dart down to the door of their hives with a precision which is generally unerring, but, if, from any cause, they miss the opening, they are obliged to rise in the air, in order to take another observation.

If bees hear—which is a doubtful question, the old-fashioned "tanging" to the contrary—they certainly hear only what affects their welfare. Their sense of taste is also far from perfect, foul ditch-water being often preferred by them to limpid streams, or even dew, and ill-smelling plants having quite as much attraction as sweet ones; it is the quantity, rather than the quality of their food, for which they care. They are also fond of the secretion of the *aphides*, the milch-cattle of the ants.

Their sense of smell is very keen; the presence of honey they detect, even in the most carefully-concealed places. Honey-bees often, in scarce seasons, attack the bumble-bees on their return from the fields laden with honey, and force them to disgorge all they have collected. Its presence in the honey-bag must have been detected by the sense of smell. The seat of this sense is in the mouth; this Huber determined by presenting successively to all parts of the body, on camel's-hair pencils, odors especially repugnant to them. When held near the mouth, the bee started back as if annoyed. On one occasion he mixed honey with camphor, which they especially dislike; by some means they managed to separate and remove all the honey, leaving the camphor untouched.

The sense which seems to be most perfect in these little creatures is that of touch, and that seems to reside wholly in the antennæ. Greetings, caresses, and the communication of intentions, are always effected, by one bee toward another, by crossing their antennæ. It must be remembered that no light enters a hive under ordinary circumstances. "The bee," says Huber, "constructs its comb in darkness; it pours its honey into the magazines, feeds its young, judges of their age and necessities, recognizes its queen, all by aid of its antennæ, which are much less adapted for becoming acquainted with objects than our hands. Therefore, shall we not grant to this sense modifications and perfections unknown to the touch of man?"

In order to determine the means of communicating ideas which exist among the bees, Huber divided a hive into two parts by a fine wire grating; the bees on both sides of the grating continued their work tranquilly, collecting honey, storing pollen, and building no royal cells. It was again divided by two parallel wire gratings, which admitted communication by means of every other sense except touch. The queenless half of the swarm fell into their usual distress and agitation, and soon set about constructing royal cells. By other experi-

ments he determined the fact that these insects recognize their queen, and communicate the fact of her presence, by the crossing of their antennæ with those of other bees. The effect produced by the amputation of the antennæ has been already described, and shows of what vital importance this organ is to them.

Huber made many experiments upon the respiration of bees, proving them to absorb oxygen, and emit carbonic acid. A number of live bees expired instantly in mephitic air; but others, when exposed to it in a torpid condition, were unaffected, showing that it was *respiration*, not *contact*, which caused their death. Two stigmata, the respiratory organs of insects, were discovered by Huber in addition to the two already known. Eggs and larvæ, he showed, also absorb oxygen and evolve carbonic acid. The air of the hives was repeatedly examined by Huber, and found to be very nearly as pure as the outside air. This fact directed his observations more closely to the economy of the hive, and he discovered that the bees maintained a systematic ventilation. Files arrange themselves in lines radiating from the door, and, by keeping their wings in rhythmical motion, generate currents of air. If the hive be sealed up, the bees at first fan violently, but in forty-five minutes the whole swarm lies apparently dead from suffocation.

It is necessary to pass over the curious provisions made by them against their enemy the *Sphinx atropos*, and Huber's discovery of the origin of propolis, to the last point which he has made luminous by his observations, the origin of wax, and its use in bee-architecture. Réaumur discovered that pollen-dust, when submerged in water, swells and finally bursts, and from the grains exudes an oily liquor, which he took to be the substance converted, in the body of the bee, into wax. This had been so long an accepted fact, that Huber did not doubt its correctness till an observation of Burneus's induced him to make a series of experiments upon the subject. He confined one swarm of bees, giving them only honey, and another, supplying them with nothing but pollen. Seven times new comb was found in the first hive and removed, while none at all was made in the second. Wax is secreted like fat in other animals, from saccharine matter, and it accumulates, in layers of delicate white spicules, in the wax-pockets of the bees. There are six depressions, lined with a reticulated membrane, in the lower side of the worker's body, between the abdominal rings.

The reason for gathering pollen was, then, a matter of interest. It was manufactured not for food, as bees lived perfectly well without it. By close scrutiny Huber discovered that the workers swallow the pollen, and after a time regurgitate it as food for the larvæ. Marked bees were seen to partake of the pollen, to ascend to the nurseries and plunge their bodies head-foremost into the cells, containing worms. After their withdrawal the cells were examined, and found to contain a supply of the jelly which constitutes the food of the larvæ.

Huber sums up the conclusions of all his experiments upon wax:

"1. That the wax comes from honey.

"2. That the honey is also a food of the first necessity for bees.

"3. That flowers do not always contain honey, as has been imagined; that this secretion is subject to the variations of the atmosphere, and that the days when it is abundant are very rare in our climate.

"4. That it is the saccharine part of the honey which enables the bees to produce wax.

"5. That raw sugar yields more wax than honey, or refined sugar.

"6. That the dust of the stamina does not contain the principles of wax.

"7. That this dust is not the food of the adult bees, and that they do not collect it for themselves.

"8. That the pollen affords the only aliment which is proper for the young, but that this substance must undergo a peculiar elaboration in the stomachs of the bees, to be converted into an aliment which is always appropriated to their sex, their age, and their wants; since the best microscopes do not show the particles of pollen or their coverings in the liquor prepared by the working-bees."

The bees, when wax is needed, gorge themselves with honey, and hang suspended in festoons or curtains for about twenty-four hours. During this repose, which Réaumur supposed was for rest and recuperation, the honey is digested and the wax makes its appearance partially under the overlapping rings of the abdomen. No other organ for the secretion of wax was found by the exquisite dissections of Mdlle. Jurine, or any of her successors, except the cellular lining of the pockets.

These scales a worker disengages by means of the pincers on its legs, and seizing the scale in its mouth. "We remarked," says Huber, "that with its claws it turned the wax in every necessary direction; that the edge of the scale was immediately broken down, and the fragments, having been accumulated in the hollow of the mandibles, issued forth like a very narrow ribbon, impregnated with a frothy liquid by the tongue. The tongue assumed the most varied shapes and performed the most complicated operations; being sometimes flattened like a trowel, and at others pointed like a pencil; and, after imbuing the whole substance of the ribbon, pushed it forward into the mandibles, where it was drawn out a second time but in an opposite direction."

These particles of wax thus rendered adhesive, ductile, and opaque, by working in the mouth, were applied to the vault of the hive. A wall of wax was begun in an inverted position, depending from the top of the hive, by this bee, which is called the founder-bee. When its store of wax is exhausted, another bee follows and proceeds in the same way, guided by the work of its predecessor. When the wall was nearly an inch in length, and about two-thirds as high as the

depth of an ordinary cell, the bees began excavating a cell on one side, and two on the other; these cells were so arranged that the partition-wall between the two cells was exactly opposite the middle of the one.

Bees, Huber tells us, do no truly coöperative work; the only thing which looks like coöperation is the unanimity with which the whole swarm waits till one bee has laid the foundation. Each bee follows the suggestion of the one which has preceded it. As the work progresses, it becomes possible for a larger and larger number to join in, and it is only the foundation-cells which are excavated; the others are built in their permanent form.

The much-praised exactness of the bee is shown to have been over-estimated; but the variations which we find in the hive are much more extraordinary than the uniformity. These are always due to something wonderfully like the intelligence of man, in its power of conforming to circumstances.

Only once in his life was Huber turned aside from his peculiar work. In his investigation upon the ventilation of the hive, he had occasion to introduce some seeds, and watch their germination. At the suggestion of Senebier, whom he had associated with himself in these particular experiments, he turned his attention to the phenomenon of germination, and, in connection with him, prepared a paper entitled "Mémoire sur l'Influence de l'Air dans le Germination des Grains," Geneva, 1801, but he soon returned to the work of his life.

No more striking commentary can be made upon the extent of Huber's labors than that afforded by a consideration of the work of his successors. The German apiarian Dzierzon has cleared up the mystery of the drone-laying queens—a mystery fully recognized and clearly stated by Huber. Many facts have been added to those discovered by Huber, and some few corrections of his statements have been made; but it has fallen to the lot of few naturalists to leave behind them a work so full and accurate as his. All that has been done in this department since his time—and, altogether, it falls short of the work performed by him—is merely a building upon his foundation.

The discoveries which he made are recorded, in full, and are supported by experiments, described with such lucidity, that to read them is almost like witnessing the facts. His clear, unerring intellect, penetrating through all side-issues, seized the gist of every difficulty; and those which he did not finally solve, he stated with such accuracy as to direct the observation of his successors.

CORRESPONDENCE.

ALLEGED FALLACIES OF SCIENCE—CAUSE AND EFFECT—MODES OF FORCE.

To the Editor of the Popular Science Monthly:

DR. JOHN W. DRAPER'S "History of the Conflict between Science and Religion" should be read by every searcher after truth. But Dr. Martineau's address in London, last fall, on "Religion as affected by Modern Materialism," considered in connection with Dr. Draper's book, portends danger to the dogmatic assertion of mere hypotheses, or guesses, as scientific facts. Enlightened people are fond of sensation as a pastime, but, in the serious and desperate conflict which is surely approaching, they will accept neither superstition nor sophistry, but only mathematical truth or pure logic. Scientific imagination, useful in research, should not be allowed to culminate in illogical speculation. Phenomena are effects: and not their causes, but only their correlates or conditions under which they occur, lie in the field of physical science.

Physical force is not the *cause* of physical action. Specific modes of force have no physical existence, *but are only in thought* correlatives to phenomena. Gravity is not the cause of the persistent movement of cosmical bodies, nor of their relative positions to each other, nor is it a physical "thing;" it is only one of the many correlates in thought, of the ever-changing relative positions of these bodies.

Physical force or pressure has no generalized mechanical equivalent. Like quantities of mechanical effect, due to like quantities of any mode of force, *are limited to exact conditions.*

The fact that mere physical force or pressure is not the *cause* of motion, and has no quantitative equivalent in mechanical effect, is mathematically demonstrated thus:

Under the gravity or pressure of the atmosphere—fifteen pounds to the inch—water has a velocity of forty feet per second, and steam, or other gas of like density, a velocity of 1,600 feet. Hence, considering the relative velocities and masses, the mechanical effect observed with reference to the gas is forty times that with reference to the water, *both substances acting under the same pressure during the same time.* (Space, never having been alleged to be a *cause*, is purposely neglected in this calculation. As a correlate of phenomena, it will form the subject of a future communication.) These varying effects obtain whenever unlike masses move under *like qualities* of constant force. Therefore, as cause equals effect, the mechanical effects, proportional to mass, being quantitatively different under the same force, they cannot be caused by the pressure or force under which they act.

Hence, that scientific imagination which treats modes of force as causes, and from this hypothesis, and exactly conditioned experiments, generalizes the quantitative equivalent effect of any one mode—as the "mechanical equivalent of heat"—and from similar hypotheses calculates the densities, temperatures, etc., of other worlds than ours, is not in accordance with mathematical demonstration, and is, therefore, a delusion. "Promise and potency" are causes, and, as Prof. Tyndall very properly says, are discerned in matter; and, being *absolutely known*, they are not susceptible of proof. But their effects, being only observed, and therefore conditioned, are susceptible of both proof and prediction from knowledge of the several conditions under which they arise. Pure science adduces not the causes, but only these conditions of phenomena.

The times portend a crisis in one of Mr. Herbert Spencer's rhythms which has long been setting in, laden with sophistry, miscalled expediency, and general demoralization. Let pure science, resolutely, but carefully, take advantage of its ebb.

A. ARNOLD

TENAFLY, N. J., *December* 28, 1874.

EDITOR'S TABLE.

TYNDALL AND HIS REVIEWERS.

WE print in full the masterly reply of Prof. Tyndall to the attacks of his critics, which is prefixed as a preface to a new edition of the Belfast discourse. It was not to be expected that he would remain passive under the unscrupulous assaults to which he has been subjected; nor that, when he did speak, he would make any half-way work with his assailants. Our readers will agree that they have got no more than they deserve; and we think that every competent reasoner must admit that Prof. Tyndall's rejoinder to the main charges against his address is conclusive.

In regard to the wisdom of opening and pursuing this important question, there can, we think, be no serious doubt. It can be condemned only by condemning the general desirableness of discussion, the analysis of opinions, and the comparison of conflicting views. It has been wisely said that of the three states of mind, or stages of conviction—the unanimity of the ignorant, the disagreement of the inquiring, and the unanimity of the wise—the second is at all events the parent of the third. He who drags people out of the slothfulness and stagnation of ignorant unanimity, even though thinking engenders discord and dispute, is doing a wholesome and necessary work. This is what Prof. Tyndall has very successfully accomplished. If to concentrate public attention upon a subject of great and acknowledged importance, to summon the most powerful minds to its re-examination, and to secure the keenest scrutiny into all its aspects and bearings, be the way to arrive at its clearer understanding, then has the author of the Belfast address done an eminent service to his generation. Such services are always useful, but they become of high and especial value when the problems brought forward are new, or are old problems which have acquired new meanings by a change of the circumstances in which they are considered. The critics of Prof. Tyndall tell us that he has raised a very old question, one which comes up alike in every age, which is no nearer a settlement now than it was thousands of years ago, and which is just as insoluble for modern science as for ancient theology. But it is not easy to understand how the mere calling up of an obsolete and hopeless question, that derives no new significance from the present state of knowledge, should have made so profound an impression upon the strongest minds, in widely-separated countries, and in this age of absorbing intellectual activity. A startling statement may arrest momentary attention, but, if empty and futile, why should its interest be so sustained? For three months after the delivery of Tyndall's address we were deluged with comments, dissections, exposures, and refutations by the daily and weekly press, and, had it been as vacant of vital meaning and pertinent application as many allege, its force would long before this have been spent, and the subject would have died away as a mere superficial and transient excitement. But things have gone quite differently. The interest has increased rather than declined, and to the rattle of newspaper musketry begins now to succeed the roar of the monthly and quarterly artillery. And this for the adequate reason that new elements are at work, old questions are reshaped, and appear in new relations, while the controversy takes on an aspect that it never presented before, and requires to be searched and sifted

in all its issues and implications. The stage of uninquiring agreement has been passed, discussion has elicited a wide diversity of opinions, but the ultimate tendency cannot fail to be toward a more enlightened harmony of views. The critics of Prof. Tyndall of course differ with him, but their differences among each other are no less marked, and their positions are often mutually destructive of each other. It will be instructive to call attention to some of the indications of conflicting opinion and converging advancement, exemplified by the later and more carefully-considered criticisms.

We have now before us three ably-written articles, called out by Prof. Tyndall's address: one in *Blackwood's Magazine* for November, entitled "Modern Scientific Materialism;" another in the *Penn Monthly* for December, by R. C. Thompson, who aims to answer the question, "What would Tyndall be at?" and a third in the January *International Review*, entitled "Ideas in Nature overlooked by Dr. Tyndall," which was contributed to that periodical by Dr. McCosh.

The first thing that strikes attention in perusing these papers is their substantial agreement in regard to the doctrine of Evolution. Dr. McCosh says: "Two great scientific truths have been established in this century. One is the doctrine of the conservation of energy. . . . the other great doctrine is that of development, acknowledged as having an extent which was not dreamed of till the researches of Darwin were published." The writer in the *Penn Monthly* is less explicit, but he assumes the principle, and would have no quarrel with it under a theistic interpretation. In fact, it is this which he contends for, and, without denying the process, is only inclined to belittle it. Of man he says: "His animal nature may or may not have owed its existence to the same process of Evolution that has brought forth each higher species from that below it. We think the question not worth a half of one per cent. of the ink and paper that have been wasted upon it. The motive of many, if not of most, of the denials might fairly be traced to a certain Neoplatonist contempt of the animal creation, which has no right to shelter itself behind the Bible. Moses's story of the origin of our animal nature is humbling enough; not less so if we construe his words as declaring its direct creation from the dust, than if we suppose that it passed through more elevated forms of existence before it attained its uprightness of stature and dignity of position. If Mr. Darwin teaches us the reality of our kinship on one side with the lower forms of life, and stirs in our hearts the feelings that that kinship should excite, he will not the less, but the more, fit us to claim a higher kinship with Him who giveth grace to the humble." Friend Thompson may be unhesitatingly "counted in" on the Evolution question, for he has evidently "conquered his prejudices" on the point of a low animal ancestry, and when this is done all the rest is comparatively easy. The writer in *Blackwood's Magazine*, so far from finding difficulty with the doctrine, takes to it admiringly. He says: "We have no quarrel with the evolutionary hypothesis in itself. It is an inspiring conception to look upon Nature in all its departments as intimately linked together from 'primordial germ' to the most fully-developed organism—from its rudest speck to its subtlest symmetry of form, or most delicate beauty of color. The idea of *growth and vital affinity* is, we readily grant, a higher idea than that of mere *technic* after the manner of men. There is no call upon us to defend the imperfect analogies by which past generations may have pictured to themselves the works of Nature."

This is a large concession, and indicates an immense step forward in man's

view of the nature of the surrounding world. And it well illustrates the three phases of opinion to which we have referred, for there was first a long unanimity of ignorance, then a stormy and obnoxious conflict, and this has led to a new and more intelligent basis of agreement. But, while there is a virtual accord among our writers as to the doctrine itself, they disagree radically as to its interpretations, and show us that there must be a good deal of warm work yet before old beliefs are brought into consistency with the new theory. The writer in the *Penn Monthly* refuses to modify his notions about breaks and new beginnings in the order of things. He says: "If there be one word more intolerable than another to science, it is *beginning*. To disprove supposed beginnings, to show that they were the outcome of what went before, is the scientist's vocation. The category of cause and effect becomes, through long practice, his first law of thought, the groove of all his mental operations. With whatever fact he is brought face to face, his first impulse is to apply that category 'to account for the fact,' as he calls the process. And when he speaks of causes he comes to mean only secondary causes, those that are themselves effects. On the other hand, this word *beginning* seems to us to embrace in it all that the metaphysician, the theist, and the Christian, have to fight for against the naturalist." But this conception of the government of the world, "all that the theist and the Christian have to fight for," the writer in *Blackwood* regards as a very derogatory view of the divine working. He says of scientific men: "It is impossible for them, or for any, to conceive too grandly of Nature, or of the unbroken harmony and continuity of its movements. The very magnificence of its order is only a further illustration of Divine wisdom; for surely the very thought of a Divine mind implies the perfection of wisdom, or, in other words, of order, as its expression. The more, therefore, the order of Nature is explained, and its sequences seem to run into one another with unbroken continuity, only the more and not the less loftily will we be able to measure the working of the Divine mind."

Again, these writers come into sharp collision over the question of the atomic theory. There has been much complaint that Prof. Tyndall did not take up for discussion some special scientific topic which he had made his own; yet he did exactly this thing. His discourse is a monograph on that part of physical philosophy which he has been compelled during all his scientific life to study, that is, the evidence and import of the doctrine of the atomic or molecular constitution of matter. This is a problem which scientific men cannot evade: they are driven to it by the very exigencies of mental action; as Dr. McCosh well observes: "We seem to be obliged by a sort of necessity of thought or speech to fall back on some such conception. If every thing we see in the world be composite, and capable of analysis and division, we have to think and talk of something indivisible and undecomposable, which we may call particles, molecules, or atoms." But if the idea is thus fundamental and deals with the very essence and core of scientific philosophy, Prof. Tyndall certainly did not go out of his sphere in considering it. And though he is condemned, there appears to be no common ground for censure. His critics are as much at variance with each other as they are with him. The writer in the *Penn Monthly* attacks the atomic theory at the outset as if it were some sort of a religious enemy which must be got out of the way; and he scouts it as an unprovable hypothesis, bad metaphysics, and which explains nothing. On the other hand, the writer in *Blackwood* declares it to be "a perfectly valid theory, resting on its own

evidence," and adequate to explain the physical origin of the universe; while Prof. Clerk Maxwell, according to Dr. McCosh, "discovers, in the very nature and properties of a molecule, a proof of design," thus making the atomic theory a help to religion by furnishing evidence of the existence of God.

It is a noteworthy circumstance, as showing the growth of a better state of mind, that the writers we are considering agree in abstaining from the charge of materialism, which has been so freely indulged in by others against Prof. Tyndall. They know that it cannot be maintained; but, while refraining from the imputation of "gross materialism," it is still implied that he must be some sort of a materialist. The writer in the *Penn Monthly* expressly acquits him of the charge as usually construed, by saying, "Prof. Tyndall is not a materialist of the school of De la Mettre and Holbach." He then puts the question, "In what sense, then, is Prof. Tyndall a materialist, if he be one at all?" and replies: "In the sense of being a *naturalist*;" and this term is again used in a vague and unusual sense. But it were better to have allowed Prof. Tyndall to explain his own position, which he has done in the most explicit manner. It is now generally understood, as the writer just quoted implies, that the term "materialism" is used with different significations, and Prof. Tyndall has qualified the form of it which he maintains as "scientific materialism." This consists simply in ascribing higher powers and possibilities to matter than hitherto, and not in sinking mind in matter, or in asserting the materiality of mind in the name of scientific authority. In an address, delivered before the mathematical and physical section of the British Association held in Norwich, in 1868,[1] Professor Tyndall took exactly the same ground that he assumed last August at Belfast; and passages from the discourse were widely quoted at the time as containing the most decisive disavowal and disproof of materialism in its usually accepted sense. Our reviewers should have reproduced the following portion; and, as they have not, we supply the omission:

"The relation of physics to consciousness being thus invariable, it follows that, given the state of the brain, the corresponding thought or feeling might be inferred; or, given the thought or feeling, the corresponding state of the brain might be inferred. But how inferred? It would be at bottom not a case of logical inference at all, but of empirical association. You may reply that many of the inferences of science are of this character; the inference, for example, that an electric current of a given direction will deflect a magnetic needle in a definite way; but the cases differ in this, that the passage from the current to the needle, if not demonstrable, is thinkable, and that we entertain no doubt as to the final mechanical solution of the problem. But the passage from the physics of the brain to the corresponding facts of consciousness is unthinkable. Granted that a definite thought, and a definite molecular action in the brain, occur simultaneously; we do not possess the intellectual organ, nor apparently any rudiment of the organ, which would enable us to pass, by a process of reasoning, from the one to the other. They appear together, but we do not know why. Were our minds and senses so expanded, strengthened, and illuminated, as to enable us to see and feel the very molecules of the brain; were we capable of following all their motions, all their groupings, all their electric discharges, if such there be; and were we intimately acquainted with the corresponding states of thought and feeling, we should be as far as ever from the solution of the problem, 'How are these physical processes connected with the facts of consciousness?' The chasm between the two classes of phenomena would still remain intellectually impassable. Let the consciousness of *love*, for example, be associated with a right-handed spiral motion of the molecules of the brain, and the consciousness of *hate* with a left-handed spiral motion. We should then know when we love that the motion is in one direction, and when we hate that the motion is in the other; but the 'why?' would remain as unanswerable as before.

"In affirming that the growth of the body

[1] This interesting discourse has been added as an Appendix to the last American edition of the Belfast Address.

is mechanical, and that thought, as exercised by us, has its correlative in the physics of the brain, I think the position of the 'materialist' is stated, as far as that position is a tenable one. I think the materialist will be able finally to maintain this position against all attacks, but I do not think, in the present condition of the human mind, that he can pass beyond this position. I do not think he is entitled to say that his molecular groupings and his molecular motions *explain* every thing. In reality, they explain nothing. The utmost he can affirm is the association of two classes of phenomena, of whose real bond of union he is in absolute ignorance. The problem of the connection of body and soul is as insoluble in its modern form as it was in the prescientific ages."

And so it turns out that he who has been buried under a mountain of execration for using science to drag the world into the abyss of materialism, is precisely the man who has demonstrated that no possible extension of science can ever lead one step toward that dread abyss. He has taught us that if science could attain perfection and predict the movements of all the atoms of Nature for thousands of years to come, as it now predicts eclipses, it would not be one whit nearer the solution or explanation of the mystery of the relation of mind and matter than it was in its infancy.

Referring to the admission in the foregoing passage, that "we cannot see any nexus between cerebral action and thought, or discover why a movement of the brain should lead to mental exercise," Dr. McCosh says, "But this was never intended to mean much." What right has Dr. McCosh to assume that Prof. Tyndall means less or other than what he says? His words are certainly not obscure, and we think they are weighty with meaning; so weighty, that it is only by an imputation of insincerity or equivocation that their effect can be escaped. Had they been generally heeded, or had Prof. Tyndall's reviewers been candid enough to make them widely known, we should have heard a great deal less vituperation of the Belfast address.

SCIENCE AND SOCIAL REFORM.

It is well known that the ground taken in this periodical in regard to the scope and influence of science is, that both as a mental method, and by the actual knowledge it furnishes, it is destined in the future to exert a growing and powerful control over public questions which have hitherto been but little, if at all, affected by it. The frantic efforts made by many to keep science in its old physical grooves, and prevent its "encroachments" upon departments of thought thus far dealt with by non-scientific methods, are doomed to certain failure. An excellent exemplification of this tendency is now furnished by the woman question. It has latterly come into prominence in various aspects as a practical reform, and the most radical and momentous changes are demanded, both in the view to be taken of the feminine nature and capacities, and in the social and public regulations to which women have been amenable in the past. The promoters of this alleged reform are generally philanthropists, sentimentalists, and politicians, who, starting from existing and acknowledged evils in society by which women suffer, rush on to the advocacy of sweeping changes, as if society had but to swallow their panaceas, and its evils would disappear. That there is a scientific side of the subject, of the greatest importance, these people never seem to suspect. It is observable that into the literature of the movement science has, thus far, hardly intruded, and little disposition is evinced to seek its assistance. We cannot, however, expect that people will be very eager to turn their backs upon their own methods of thought; and our professed reformers have their own well-settled methods. In the present case, certain political assumptions are made, and certain beliefs postulated, regarding feminine character, and from these a reformatory policy is deduced and

hotly urged for immediate adoption. This was the course of even so trained a thinker as Mr. Mill. He brought his resources of philosophy and logic to bear upon the subject, and gave to the reformers their text-book; but he went on as oblivious of science as if such a thing had never been heard of. And yet the fundamental questions of this important movement belong solely to scientific investigators. Politicians, philanthropists, and logicians, will grope blindly and strike wildly in treating it, until Science has instructed them in its phenomena and laws, and shown them what voice Nature has in the decision of their questions. Politicians do not seek this information, nor care for it, when it is thrust upon them; but, all the same, the question must finally be determined by it. As we have said again and again, the missing factor, in its current discussion, has been a scientific exposition of the peculiarities of the feminine nature, and this absent factor is at the foundation of the whole inquiry. But the discussion is already beginning to turn in the scientific direction. The appearance and large circulation of Dr. Clarke's books, and the perturbations and reactions they have produced, show that this bearing of the subject is beginning to be appreciated. We have recently published two excellent contributions, written by ladies, which quietly assume the scientific point of view, and recognize its controlling importance. They even treat this delicate and serious subject in the light of the most advanced speculations, against which many have an intense repugnance, and thus testify that the question can only be settled on the basis of reason, fact, and natural law. Other essays have been sent us with varying merits, which we have been compelled to decline for want of room, and because we aim to represent that scientific side of the subject which fails to find expression in other magazines. And now we ask the careful attention of our readers to the article of Dr. Van De Warker, in the present MONTHLY, on "Women in Relation to the Professions and Skilled Labor." The mode of considering the subject adopted by this writer is what has long been wanted; and his facts and conclusions should be well pondered by those who are vehemently advocating "revolution" in the social and industrial relations of women. Philanthropy is an excellent thing if duly enlightened; measures of relief are desirable if wisely conformed to facts; and therefore the first thing is to hear what Science has to say as to the fundamental conditions upon which all genuine and permanent reform must depend.

AN ELECTRO-MEDICAL HUMBUG.

THERE appeared, in THE POPULAR SCIENCE MONTHLY of February, 1873, an article entitled "Is Electricity Life?" taken from the English *Belgravia Magazine*. Its admission to our pages was an editorial inadvertence, the article having been glanced at in haste, and only the first portion of it read. Its object, however, was to puff a quackish device of magnetic chains and bands, to be worn for the cure of nervous diseases. They were first called "Pulvermacher's Rings," and, having now been revived as "The Voltaic Armadillo," they are advertised as indorsed by THE POPULAR SCIENCE MONTHLY. The advertiser says that the most eminent medical men of Europe and America approve their use, but none of their names are given, the sole authority quoted being the foreign writer in this magazine. Now, the publication of that article was a blunder; and the article itself is worthless and absurd: and if all editors, who happen to have been, at some time, the victims of careless oversight will copy this paragraph, they may help to protect a great number of stupid people with "rheumatics" and "neurology" against being humbugged.

LITERARY NOTICES.

THE NATIVE RACES OF THE PACIFIC STATES OF NORTH AMERICA. By HUBERT HOWE BANCROFT. Vol. I. Wild Tribes. 797 pages. Price, $5. D. Appleton & Co.

IF it be true that "the proper study of mankind is man," it is equally true that mankind has shamefully neglected its lessons. There is, perhaps, no great subject upon which the knowledge attained is so scanty, chaotic, and misleading, as that relating to the different sorts of humanity, or the races and varieties of mankind. Into the cause of this it is not necessary here to enter, but it is probably connected, in an intimate manner, with the laws of growing intelligence. Until there arises some perception of the value of knowledge, of the relation of facts to principles, and the importance of valid generalizations, there will be no such thing as accurate, methodical observations, and the systematic collection of the data requisite for the formation of intelligent opinions. The unfilled gaps in "Spencer's Tables of Descriptive Sociology" give us the most striking illustrations of the deficiency of trustworthy information regarding the characters, habits, and peculiarities, of the different tribes of men. It is a matter of great importance that these deficiencies should be supplied, and the prominence which ethnological studies have latterly assumed, as a part of the general progress of science, gives assurance that the subject will be less neglected in the future. We have reached a stage in the growth of knowledge concerning the social relations of men which makes it necessary to have the elementary facts exhaustively collated, carefully digested, and thrown into conveniently-accessible forms for general reference and study.

This necessity has been distinctly seen by the author of the work before us. For many years a resident of San Francisco, in the midst of decaying races and the relics of old civilizations, he was attracted to ethnological problems, and saw the importance of making the subject a matter of comprehensive study. He has devoted twenty years to this task, the result of which is a work of encyclopedic scope, the first volume being devoted to the wild tribes of the Pacific region of North America, and this is now published. The second volume will treat of the civilized nations, to be followed by three volumes on the Mythology, Languages, Antiquities, and Migrations of the races and tribes that are embraced within his scheme. Of the thoroughness with which Mr. Bancroft has carried on his work, the following extracts from his preface give a good intimation:

"To some it may be of interest to know the nature and extent of my resources for writing so important a series of works. The books and manuscripts necessary for the task existed in no library in the world; hence, in 1859, I commenced collecting material relative to the Pacific States. After securing every thing within my reach in America, I twice visited Europe, spending about two years in thorough researches in England and the chief cities of the Continent. Having exhausted every available source, I was obliged to content myself with lying in wait for opportunities. Not long afterward, and at a time when the prospect of materially adding to my collection seemed any thing but hopeful, the 'Biblioteca Imperial de Méjico,' of the unfortunate Maximilian, collected during a period of forty years, by Don José de Maria Audrade, *litérateur* and publisher of the city of Mexico, was thrown upon the European market, and furnished me about three thousand additional volumes.

"In 1869, having accumulated some sixteen thousand books, manuscripts, and pamphlets, besides maps and cumbersome files of Pacific coast journals, I determined to go to work. But I soon found that, like Tantalus, while up to my neck in water, I was dying of thirst. The facts which I required were so copiously diluted with trash, that to follow different subjects through this trackless sea of erudition, in the exhaustive manner I had proposed, with but one lifetime to devote to the work, was simply impracticable. In this emergency, my friend Mr. Henry L. Oak, librarian of the collection, came to my relief. After many consultations, and not a few partial failures, a system of indexing the subject-matter of the whole library was devised, sufficiently general to be practicable, and sufficiently particular to direct me immediately to all my authorities on any given point. The system, on trial, stands the test, and the index, when completed, as it already is for the twelve hundred authors quoted in this work, will more

than double the practical value of the library."

Of the spirit in which the work has been executed we may say that the writer's main object has been to make it trustworthy and valuable for the use of students. He is evidently a most painstaking and conscientious worker, and is constantly careful to give the authorities for his statements. Although not without his own views upon the various questions which arise, he is not dogmatical, and puts his reader in full possession of the grounds upon which his judgments are formed. It may be added that, though the present volume contains a great amount of information which might be supposed to be dry and unattractive, its pages are nevertheless extremely readable; and, to those who have any interest in ethnological inquiries, they will have a strong fascination.

REPORT OF THE COMMISSIONER OF AGRICULTURE FOR THE YEAR 1872.

THE recent reports of the Department of Agriculture show, we think, considerable advance on their predecessors. The Statistical Division is the work of six thousand collaborators. The entomological report, by Townsend Glover, who is himself scientist, artist, and engraver, is a really able exhibit of the *Diptera* or flies, from the great bot-flies, to the diminutive wheat-midge, which caused a loss in the cereals to the State of New York in 1854 of $15,000,000! Ryland Brown, the chemist, gives analyses of vegetables, soils, and fertilizers, etc. The report of George Vasey, the botanist, contains well-worked papers on arboriculture, especially on the cinchona, a tree which yields the Peruvian bark, and from which is obtained quinine. Thomas Taylor details exhaustive work as a microscopist on the fungi which produce the pear-blight, and the yellows in the peach-tree. The epizooty, fish-culture, and forest cultivation on the Plains, receive attention. There is also a good report on industrial education, etc., etc. The whole report gives evidence of painstaking work in the field of agriculture, and the results given are practical to a degree. The article by Prof. James Law on "Influenza in Horses" deserves circulation as a separate document.

INSANITY AND DISEASE. An Address before the Oneida County Medical Society, October 13, 1874. By L. A. TOURTELLOT, M. D., Utica, New York.

THE question of the relation of mind and body, while it has a profound interest for the speculative thinker, takes on a more practical aspect to the physician. Yet it is true that this practical urgency of the problem to the medical man is the chief cause of the more rigorous scientific investigation which the subject has received in the present century, and which has revolutionized the older views that were entertained concerning it. But the question may be properly raised whether this revolution has not proceeded too far, and led to indefensible extremes of doctrine as a reaction from the absurdities of the older belief. We understand this to be substantially Dr. Tourtellot's ground in this able address. He says: "During the middle ages and down nearly to the present century the secondary and inferior nature of matter as compared with mind was a doctrine almost undisputed in the world of learning. This doctrine is reflected in the sentiment which despised and contemned the human body and discouraged the studies of physiology and pathology. It followed also that bodily diseases were attributed to sin or to possession by evil spirits. Mental disorder especially was considered a disorder of the soul, and moral insanity was thought to demand punishment rather than to excuse from it. It is easy to see how such a belief operated to cause that abuse and neglect of the insane which so long prevailed. Its evil effects upon society also appear in the monastic rules, the severe penances, and the thousand extravagances of religious fanaticism which history records."

The reaction from this doctrine, beginning in that higher respect for matter which modern science inculcates, professes to base itself upon the results of physiology and pathology, and propounds the view that mental disorder is in all cases simply a bodily or brain disease. The implication is that mind, in its normal or usual manifestation, is a cerebral function, and this is held to necessitate the inference that abnormal mental manifestations are caused only by physiological derangement. This is the ground taken by that eminent alien-

ist Dr. Maudsley, and Dr. Tourtellot admits that, however great may be the want of evidence to establish the theory, it is at least a consistent position. Dr. Maudsley says, "To write as if sanity is a thing of the immaterial, and insanity a thing of the material world, is to infer that men are furnished with brains only that they may become insane." Yet it seems that some physicians find it convenient to entertain both theories; and we are informed in this discourse that Dr. John P. Gray, Superintendent of the State Lunatic Asylum at Utica, in a paper read before the New York State Medical Society, on "The Dependence of Insanity on Physical Disease," takes the ground that mental aberration is due to bodily disorder; but, being "no materialist, he does not regard sound mental action as the result of a sound and healthy brain, and denounces such a notion" as "an attempt to revive the exploded vagaries of the French materialism of the encyclopedists and the Revolution." Obviously the superintendent is a politician as well as a doctor.

But, even assuming that insanity is a bodily disease, Dr. Tourtellot maintains that it is impossible to find the indications of it in the bodily structures. He quotes Leidersdorf as declaring any such demonstration "outside the realm of possibility;" while Griesinger, the celebrated German authority, ridicules the "belief that every mental disorder must correspond to a palpable cerebral lesion." The practical evil of the extreme theory, that insanity is in all cases essentially a bodily disease, Dr. Tourtellot thinks to be an undue reliance upon medication for a cure. This leads to the appeal for legislative aid upon an enormous scale, to provide medical establishments for the treatment of the insane. Upon this point Dr. Tourtellot remarks:

"I must point you to one other evil, of perhaps greater magnitude, which is directly due to Dr. Gray's false theory and vicious reasoning. You are doubtless aware that the demand for new hospital-asylums, to provide for all the insane of the State, has been based mainly upon the doctrine that insanity is a bodily disease, easily curable in its early stages. At length, the Legislature of this State was prevailed upon to authorize the building of two such asylums. And, as they were in the end to save vast sums of money through the speedy cure of those who must otherwise, as incurable, become a public charge for life, it was not necessary to spare expense upon them. These asylums, in fact, were planned to require an outlay of from $8,000,000 to $10,000,000 for a total capacity of 800 patients. Now, I need not stop to prove to you how even more absurd is the plan of 'stamping out' insanity by medical treatment than that of exterminating cancer and scrofula would be. The serious proposal of such a scheme by a medical man in any part of Europe would be enough to brand him as a visionary or a quack. That it should not only have been proposed, but apparently accepted as the basis of a policy of provision for the insane in this country, seems hardly credible. But, as you know, the policy is still urged upon our State Legislatures, and the argument for it is repeated in nearly all our asylum-reports, year after year."

Dr. Gray makes the statement in his last report to the Legislature, that "a recovery of four-fifths" (of insane persons) "might reasonably be expected, if treated within three months of the first attack; while, if twelve months are allowed to elapse, the same proportion may be considered as incurable." Dr. Tourtellot replies that this is a fallacy, and states that, in a large number of the cases, the outbreak is sudden and transient, and that "it is quite certain that four-fifths of such patients will recover even without special treatment. . . . But, on the other hand, patients brought to the asylum a year or more after their attack, are of a wholly different class. Their insanity was chronic, not only in its fully-developed stage, but in the stage of invasion. It was never possible to bring them to an asylum within three months of the date of their attack. Their insanity came on imperceptibly, and opinions might differ months or even years as to the time of its beginning."

In regard to the vexed question of the definition of insanity, the author remarks:

"After what has been said, you will not expect me, in closing, to present you with a definition of insanity as a physical fact. The term is a purely metaphysical one, and cannot be translated into the language of natural science. To say that insanity is morbid energy of the nervous element, is to state what we all admit to be a legitimate scientific hypothesis, but at the same time know to be worthless as a definition. On the other

hand, a precise definition from the side of mental science is acknowledged to be impossible. Taken in its broad sense, the word simply denotes a marked degree of mental disorder. What that degree is, depends upon the practical issue presented in each case. In highly-civilized countries a moderate degree only, more or less incapacitating for the due performance of the social relations, constitutes insanity, and entitles its subjects to the charity and protection of the state. Where it is claimed to excuse from the punishment of crime, a high degree of mental disorder is required to be shown. Other degrees are necessary in order to make void a will, or excuse from the performance of a contract. A slight degree only may constitute a medical case of insanity, of the greatest interest and importance. In a narrower and technical sense, insanity denotes chronic mental disorder not obviously belonging, as a symptom, to some recognized form of bodily disease."

ANIMAL PHYSIOLOGY: The Structure and Functions of the Human Body. By JOHN CLELAND, M. D., F. R. S. New York: G. P. Putnam's Sons. 12mo, 325 pages. Price, $1.50.

IN the first chapter of this work the author gives a brief account of the relations of physiology to other departments of science; a description of the composition and properties of organic matter; definitions of the principal functions; and a couple of pages on the neucleated corpuscle, which he is disposed to regard as the ultimate physiological unit. Connective tissues; the skeleton and its function, with the minute structure of bones, ligaments, and cartilages; muscles, their structure and mode of action; and the structure and functions of the skin and mucous membrane, form the subjects of the four succeeding chapters. Alimentation, and the apparatus and process of digestion, are next treated; and then follow in the usual order chapters on the blood, the circulation and its organs, and respiration. The structure and functions of the glands are next disposed of; and three chapters are given to the anatomy and physiology of the nervous system, including the organs of special sense. Voice and speech are next treated; and the book closes with a chapter on the subject of reproduction and development. The matter of the work is largely anatomical, too much so, in our opinion, for a book on physiology; but, as the author says that it is intended for those "previously unacquainted with anatomical details," this feature may be exactly suited to their needs. In this case, however, the physiology should have been similarly graded, which it is not, being much too advanced for pupils ignorant of anatomy. The work is mainly a compilation, the author claiming originality only in his method of grouping the facts. The style is clear, the illustrations are numerous and well executed, and both a glossary and index are appended.

ELEMENTS OF ZOOLOGY FOR SCHOOLS AND SCIENCE CLASSES. By M. HARRISON. New York: G. P. Putnam's Sons. 16mo, 172 pages. Price, 75 cents.

THE title of this book is doubly misleading. It is called a zoology, when, in fact, it deals with only one department of that great subject, viz., comparative anatomy; and, instead of giving the *elements* of this, goes rather to the opposite extreme, being little more than a bare statement of those later generalizations embodied in modern systems of classification. The book is not adapted to the wants of beginners, and is, therefore, quite out of place in an "elementary series." Those, however, desiring a brief summary of this branch of zoological science will find it of service, though the works of Huxley, from which it is mainly derived, put the subject in a much more attractive shape. The book is copiously illustrated, has a series of questions attached to each of the chapters, and is provided with a glossary.

MISCELLANY.

Observations of the Transit.—So far as heard from, the numerous expeditions which went out to observe the recent transit of Venus met with a fair measure of success. By the wise liberality of the various governments, the contingencies of fair or foul weather were provided against, and the view, which at one point was obstructed by clouds, was more successfully had at some other station in the same latitude, where the skies were more propitious. At Wladiwostock, the most northern station

occupied by American observers, the moment of first contact was accurately ascertained, despite a hazy atmosphere, and 13 photographs were taken. From Peking, which is station 2 in Prof. Langley's charts, we have as yet seen no report. At Nagasaki, the observers met with complete success. At Hobart Town, success was only partial; still 113 photographs were taken. The party whose station, according to Prof. Langley, was Bluff Harbor, New Zealand, seem to have located themselves at Queenstown, in that colony. Their observations were very successful, and "237 photographs were made of the first contact." From the remaining three American stations, viz., Chatham, Kerguelen, and Possession Islands, no report has yet been received.

Accounts from stations occupied by European astronomers report entire success at Cairo, Suez, Thebes (Egypt), Bushire (Persia), Calcutta, Rurkee, Kurrachee (India), Hiogo, Nagasaki, Yokohoma (Japan), Melbourne (Australia), Hawaiian Islands (3), and Tschita and Jalta (Russia). From twelve stations total failure is reported, and from seven partial success. The success of the American party in New Zealand is specially gratifying, as furnishing observations from a distant point in the Southern Hemisphere, to be compared with those taken near the same meridian in the Northern Hemisphere. The observers in the more remote islands of the South Sea (Chatham, Kerguelen, Possession) are not likely to be heard from for some weeks. Arrangements have been made by the British Astronomer-Royal to have dispatches from these islands forwarded at the earliest possible moment.

The zeal of the various governments in equipping expeditions for observing this transit is without parallel. Concerning the part taken in this noble strife by the United States, *Nature* observes as follows: "The United States lead all the other nations in respect both to the amount of money which her Government has contributed, and of the discomfort, not to say dangers, of the stations she has chosen in the southern seas. Posts of importance, which were given up as too hopelessly miserable even for enthusiastic English astronomers, have been occupied by Americans."

Systematic Position of the Brachiopods.—The *Brachiopoda* is a class of animals peculiar to the sea, and their remains fill the rocks of past ages Their bodies are protected by bivalve shells, which externally bear some resemblance to the shell of *Anomia* and other mollusks; and most of them live attached to the sea-bottom by a sort of fleshy stalk or peduncle. It was formerly believed that all Brachiopoda were so attached; but the genus *Lingula*, first carefully studied in the living state by Prof. Edward S. Morse on the coast of North Carolina, was found by him living free in the sand. He published a brief account of his discoveries in the *American Journal of Science and Arts;* and since then the late Dr. Stolinsky, Director of the Geological Survey of India, has confirmed his work by observing the same peculiarities in the large *Lingula anatina* in the Indian Ocean.

From the beginning, the brachiopods have been unhesitatingly classed with the mollusks—neither Cuvier, Owen, Vogt, Hancock (whose remarkable memoir won for him the gold medal of the Royal Society), Huxley, Davidson, nor others who have written on the subject, having even suggested that they belonged elsewhere. After long and industrious study, Mr. Morse, in 1870, boldly announced, in the *American Naturalist*, his belief that the brachiopods were true annelidan worms, and had not the slightest relation to the molluska. Of course, such revolutionary views were utterly denied by the conchologists in this and other countries; nevertheless, Morse persisted, and he now has the satisfaction of seeing his discoveries indorsed by many of the leading naturalists of the world. From time to time since 1870 he has published, in the "Proceedings and Memoirs of the Boston Society of Natural History," the results of his studies on the Brachiopoda, and was the first to throw light on the embryology and early stages of certain members of this class. Last year, for the first time, he gave a complete history of one of its forms, from the egg to maturity, illustrating his memoir by two steel plates, containing over one hundred figures. The discoveries there recorded fully vindicated the position he had previously maintained, that the brachiopods were annelids, and not mol-

lusks. Among the many naturalists who have indorsed this radical change in classification, Leidy, Mr. A. Agassiz, Hyatt, Packard, Barnard, Hartt, Tuttle, and Dr. Coues, may be named for this country, and Mr. Darwin, Gegenbaur, Haeckel, and others, abroad. Mr. Morse also pointed out in the above memoir that, twenty years ago, Dr. Steenstrup, of Copenhagen, had entertained the same view respecting their affinities.

Recently, Dr. Kowalevsky, the celebrated Russian naturalist, has published in Moscow a memoir on the embryology of certain Brachiopoda studied in the Mediterranean, in which he not only fully confirms the embryological studies of Prof. Morse, but indorses the latter's view, that the brachiopods are annelids. In a review of Kowalevsky's memoir, published in the last number of the *American Journal of Science and Arts*, Mr. A. Agassiz, after calling attention to the striking manner in which the investigations of this writer confirm the view of Steenstrup and Morse regarding the affinities of Brachiopoda with annelids, goes on to say: "It is not out of place to recall the very ungenerous treatment which Morse received at the hands of many conchologists for the heresies of his paper on the systematic position of the Brachiopoda; and it certainly is a striking proof of the sagacity of Morse to have announced so positively, from the history of the American Brachiopoda, the vermiform affinities of brachiopods, now so conclusively proved by the development of Argiope in Kowalevsky's paper."

A Curious Winter Climate.—Prof. Frankland has communicated to the Paris Academy of Science some curious observations made by him in the Rhetian (Grisons) Alps, and specially in two villages situated at an altitude of 5,412 feet, and much frequented by consumptives. Last December, while the soil was covered with snow, at a temperature of 24° Fahr., Mr. Frankland found the patients spending the whole day out-of-doors, in the sunshine, and wearing the same clothing they usually wore in spring and autumn. On inquiring into the cause of this, Mr. Frankland discovered that a thermometer exposed to the sun's rays showed an atmospheric temperature of from 95° to 104° Fahr., that is to say, summer heat. Providing the air is calm, living in this atmosphere is very beneficial to persons affected with chest-diseases. The author at the same time perceived that this heating of the air takes place immediately on the appearance of the sun above the horizon, and that it continues till sunset. Further, he observed that if a thermometer be placed in an inclosed area, one of the walls being of glass, and the others coated with lamp-black, the inside temperature quickly rises to 221° Fahr.

Elongation of the Trunks of Trees.—Mr. Elias Lewis, Jr., of Brooklyn, recently read a paper before the Natural History section of the Long Island Horticultural Society, giving the results of some observations on this subject. He said: "If a tree-trunk lengthens by any process of interior enlargement, it is quite certain that marks upon its surface, or lateral branches, would be carried upward as growth went on. A branch projecting at a given height from the ground would, later, become more elevated." He cited an instance of an oak-tree near Miller's Place, in Suffolk County, L. I., which is evidently over a century old, from which projects an enormous branch at a height of seven feet from the ground. This branch is thirty feet in length, two-thirds that of the tree, and is just one-half the circumference of the trunk (which is 8½ feet) where it issues. It is nearly horizontal, the inclination, which is *upward*, being very slight. At a distance of four feet from the tree, it rests upon a bowlder of great size, and spreads to a width of five feet; but the branch, in its under side, projects squarely against the face of the rock. The branch then rests on the rock about five feet, and from this point of support rises to its terminus. It is considered entirely certain that the branch began its growth when the tree was very small, and its growth has been contemporaneous with that of the trunk.

The under half of the branch, as remarked, is directly against the face of the rock, and could not have increased in length. The branch issued at about seven feet elevation, and this distance has not been increased, else, at its junction with

the trunk, it would have been carried up, which has not occurred. This is believed by Mr. Lewis to be conclusively shown in the position of the branch in respect to the rock, which surely has not changed its position; the weight of the rock, above the surface, being probably thirty tons. It is safe to conclude that the branch in question has rested on the rock, in its present position, at least a hundred years, during which time the trunk of the tree has increased in length fifty feet.

NOTES.

PROF. MARSH reached his home in New Haven, December 12th, after an absence of about two months in the West. The results of his "scientific raid" to the Bad Lands, near the Black Hills, were very satisfactory. Nearly two tons of fossil bones were collected, most of them rare specimens, and many unknown to science. There were several species of gigantic *Brontotheridæ*, nearly as large as elephants. At one place the bones were heaped together in such numbers as to indicate that a herd of the animals had been swept into a lake by a great freshet. The whole collection goes to the Peabody Museum of Yale College. A military escort, provided by General Ord, shared with Prof. Marsh the honors and the perils of the brief but brilliant campaign.

PROF. SIMON NEWCOMB, of the Washington Observatory, has gone to Europe to examine the different kinds of flint and crown glass made for optical purposes by the best manufacturers abroad. His trip is in the interest of the Lick Observatory of California, the trustees of that institution wishing to employ the best maker for the founding of the glass.

IT is announced that the *Phylloxera* has made its appearance in Switzerland, and the delegates of the vine-growing cantons are considering the best means of preventing its extension. The volcanic soil around Vesuvius is said to be an antidote to the potato-fungus, and to be of great value in the treatment of *Phylloxera*.

MR. JOSEPH WILLCOX, of the Philadelphia Academy of Sciences, has discovered in North Carolina a number of burial-places where the bodies had been placed with the face up, and covered with a coating of plastic clay. A pile of wood was then placed on top and fired, which consumed the body and baked the clay, which retained the form of the body. This was then lightly covered with earth.

THE supposed fossil remains of land-plants, found by Mr. Lesquereux in the upper portion of the Cincinnati group (Lower Silurian Rocks), near Lebanon, Ohio, are described by Prof. J. S. Newberry, in *Silliman's Journal*. These fossils are to be referred, according to Mr. Lesquereux, to the *Sigillariæ*, but Prof. Newberry shows that they do not possess the characters which belong to land-plants, and that in all probability they are nothing but casts of the stems of fucoids.

PROF. ALFRED M. MAYER, of the Stevens Institute, has shown, by a series of new and very ingenious experiments, the truth of Prof. Henry's inference that the discharge of a Leyden jar is multiple and oscillatory in its nature. Other physicists had already established this point, but it remained for Prof. Mayer to trace the oscillations and to determine the number of partial discharges per second. In one of his experiments he found the average interval between the partial discharges to be $\frac{1}{1100}$ of a second.

A TUNNEL, very nearly one mile in length, has been bored through Musconetcong Mountain, N. J., on the line of the Easton & Perth Amboy Railroad. With the exception of the Hoosac, this is the longest tunnel east of the Mississippi. The engineering-work on this tunnel was singularly accurate, the error in line being only half an inch, in levels one-fifth of an inch, in chaining or measurement six inches.

THE climate of Minnesota has been recommended as very favorable to consumptives, on the ground that the air of that region is exceptionally dry. But, from the *Monthly Weather Review* of the Signal-Office, for November last, it appears that Minnesota shows the greatest relative humidity of all the regions there named. Thus, the relative humidity for New England was .69, middle Atlantic States .66, lake region .69, Minnesota .77.

THE Caen Academy of Science offers a prize of 4,000 francs for the best essay on the "Function of Leaves in the Vegetation of Plants." The Academy does not want simply an exposition of the present state of science upon this question, but also exact experiments, performed by the competitors themselves, and new facts tending to throw light upon the present theories. The essays to be submitted before January 1, 1876.

PROF. LANDOIS reports to the Natural History Society of Prussian Rhineland that ants have the power of producing vocal sounds, though of a pitch inaudible to man. He has proved that they possess a sound-apparatus resembling that belonging to the sand-wasp.

Dr. HENRY MAUDSLEY.